HOMŒOPATHY

THE SCIENCE OF THERAPEUTICS

A Collection of Papers

ELUCIDATING AND ILLUSTRATING

THE PRINCIPLES OF HOMŒOPATHY.

BY

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TO

P. P. WELLS, M. D.

THE HONORED COLLEAGUE AND BELOVED FRIEND OF

CARROLL DUNHAM,

THIS VOLUME

IS RESPECTFULLY DEDICATED.

PREFACE.

Since the death of Dr. Carroll Dunham, a desire has been expressed by many of his colleagues that his writings should be collected in book form.

The present volume contains such as most directly express his views on the cure of disease by drugs. In their arrangement no attention has been paid to the order of time in which they were written, but those on the principles of cure have been placed together, while clinical cases illustrating these principles have been put side by side; and so great is the unity of thought running through all, that it has not been deemed inappropriate to give to the collection the title of the main Essay.

If, forgetting that experience and reflection will, as years go by, modify the opinions of every thinker, any reader should fancy he sees contradictions in these papers, he is referred to the dates at which they were written[1] and he will discover a mind ever progressive and hospitable to truth. In 1864 the Author writes: "The object of our professional life is to find out the truth, and shape our practice accordingly. Consistency to this object is true consistency,

[1] See Table of Contents.

while consistency to any form of opinion or doctrine which may at one time have been supposed to be the truth and proclaimed by us as such, consistency to such opinion merely because we may have once publicly uttered it, this is the basest and most ignoble bigotry and cowardice."

These papers were occasional, unconnected contributions to periodicals, addresses delivered at the request of societies, studies of remedies or some topic connected with them, and clinical cases. Could they have been revised for publication as a book by the hand that wrote them they might be more uniform in style though scarcely more connected in thought. To the Essay, "Homœopathy the Science of Therapeutics," the Author devoted more than usual time, and after its publication in the *American Homœopathic Review* it was issued in pamphlet form. The Dedicatory Letter which then accompanied it is here given:

To M. J. CHAPMAN, A. M., Cantab., M. D., Edin., London.

Dear Friend and Colleague:

If I show presumption in honoring these pages by the prefix of your name, I may plead in extenuation that they were in some sort written at your instigation.

When I had the pleasure of personal intercourse with you (now ten years ago), you frequently gave me the following advice: "Make a study of Hygiene in its relations to Medicine!"

One could not proceed far in such a study, involving as it does the entire philosophy of the art of medicine, without being compelled to meet the

following questions, which have perplexed, and still perplex and divide our school:

Is the Homœopathic law of cure sufficient for all the procedures required in the treatment of the sick?

Or is it, on the contrary, only a part of a still broader generalization?

Or, again, are there *other* laws which are *sometimes* to be our guides in Therapeutics? and if so, *when* are they to be used?

Are so-called "auxiliary" means ever requisite? and if so, how does this fact affect the jurisdiction of the law of cure?

Those who give only a *limited* jurisdiction to the Therapeutic law deduce their *faith* from their *practice*; but they have never given a satisfactory definition of the *boundaries* of this limited jurisdiction.

Those, on the other hand, who claim exclusive dominion for the Therapeutic law, belie their faith by their practice, for in treating the sick they do, as a matter of fact, avail themselves of means and methods which are not authorized by this law.

Yet neither party should be called dishonest. The truth is, their clinical instinct, their practical sagacity, has outrun their philosophical acuteness. Each party felt its way nearly to the true ground, but neither has succeeded in vindicating its claim to consistency in occupying that ground.

The result has been, on the part of Homœopathic practitioners, an uncertainty of their true position in relation to Old-school medicine, and, consequently, a degree of distrust on the part of the community.

It has seemed to me that these difficulties might be removed, and the perplexing questions fairly answered, by an elaboration of the nature and relations of the sciences of Therapeutics and Hygiene, which should show:

That *Therapeutics* concerns herself only with the discovery and selection of an individually specific remedy for each individual case of disease; which is done in accordance with a Therapeutic law. This law may be the Homœopathic formula, or it may be some broader generalization; but there can be but one law of this kind;

That the province of Hygiene is to discover whatever causes may have contributed to induce or perpetuate the diseased condition, and, if possible, to remove them;

That *Hygiene* alone is sufficient to restore many sick persons to health, and that it is in most cases an indispensable aid to Therapeutics; that, therefore, the practicing physician is at one and the same time Hygienist and Therapeutist, employing often, therefore, for the same case both specific remedies and various "auxiliaries;"

That in so far as Hygiene is concerned, Homœopaths and Allopaths occupy common ground,—the philosophy of the science being the same for both, however modified and shaded in practical application by the different Therapeutics of the two schools;

That in Therapeutics alone—that is, in the discovery and selection of the individually specific remedy for each individual case of disease—do we differ radically from the old school of medicine,—the Allopaths having, in fact, no science of Therapeutics whatever, *their philosophy of cure being an application of the principles of Hygiene to all diseased conditions.*

PREFACE.

The natural sequence of a study of the philosophy of the science of Therapeutics is the question how to make a practical application of it in the treatment of the sick. I have therefore written of the study of the Materia Medica.

Should the views I present throw any light upon these subjects, I shall have to thank you for the direction given to my studies. Should they prove worthless, you will join me in hoping for a sounder fruit from the labors of the next decade.

CARROLL DUNHAM.

New-York, January, 1863.

Should the reader miss in the present volume some Paper he had expected to find here, it is probably among the "Lectures," reserved for future publication, or among the Miscellanies not strictly belonging to the present book.

Irvington-on-Hudson,
June 25, 1877.

CONTENTS.

HOMŒOPATHY THE SCIENCE OF THERAPEUTICS.

Homœopathy claims to be "The Science of Therapeutics." This claim involves the assumption that prior to the establishment of Homœopathy on a scientific basis, Therapeutics, as a science, had no existence. It is incumbent on Homœopathists to show the justice of this assumption. To accomplish this, it will be necessary to prove that such a science is possible, to demonstrate what its nature and conditions must be, and to show that medicine hitherto has not furnished the means for the construction of a science which fulfils these conditions.

It is the object of this paper, first, to discuss the nature and conditions of the only possible science of Therapeutics, to show that these conditions are not fulfilled by what is called rational medicine, and to inquire in what degree they are fulfilled by Homœopathy. And, second, to examine the methods by which this science may be most successfully studied and made subservient to the practical art of medicine.

But on the threshold of the discussion we are met by the necessity of defining the term Therapeutics. It will be found that, though still sufficiently comprehensive, it is used by us in a much more restricted sense than that which, in popular thought, is attached to it.

Therapeutics being etymologically the science of curing diseases, it would seem to comprise the entire function of the physician. It is evident, however, on slight consideration, that the medical man in general practice brings into requisition too great a variety of scientific knowledge to admit of comprehension under a single term. To show how great this variety is and

at the same time to give an indirect definition and limitation to the term Therapeutics, let us analyze a series of cases, which shall not be *ideal* cases, but drawn from records of actual practice.

The physician is called in haste to an elderly person whose only intelligible complaint is of great anguish in the præcordia, and which appears by its violence to endanger his life. If he be in immediate danger of death, the state of his affairs renders it desirable that his family should be informed of the fact and how long he will probably live. The *prognosis* is demanded, and its correctness is a matter of great importance. The medical man must rely on his knowledge of Pathology[1] for a conclusion respecting the nature, cause, and probable termination of the disease. With this, if it be a fatal case, the function of the physician ends. Yet how important may this function be to the survivors! Here is as yet no question of Therapeutics. It is merely a problem in the natural history of disease, which the physician regards just as a Naturalist would a problem in Physics, and just as if the idea of curing disease had never entered the head of man.

Again, he is called to a man who lies in an epileptiform convulsion. It is well known that convulsions may arise from the most various proximate causes. They may result from physical irritation of the nervous centres or of the extremities of the nerve filaments, and in this case they will cease to recur so soon as the cause of irritation is removed; or, on the other hand, from modifications of the vital functions, such as are beyond our observation and which we can rectify only indirectly by the action of specific

[1] Throughout this paper, as always by its author, *Pathology* is used with a meaning totally distinct from that of *Pathological Anatomy*. The former is the science of *morbid processes* and *functions*, and is *abstract*; the latter is the science of *morbid organs* and *tissues*, and is *concrete*. A corresponding distinction obtains between Physiology, which is the science of *healthy processes* and *functions*, and Physiological Anatomy, which is the science of healthy *organs* and *tissues*.

agents. The first problem before the physician is to determine by a study of the phenomena which the patient presents, to which of these great classes the case before him belongs. It is indispensable to determine this question, because the treatment of the case must depend directly upon it,—in the former case it may be mechanical or hygienic, in the latter it must be therapeutic. He determines, we will suppose, in the case before us, that the convulsion is eccentric in its origin and reflex in its nature, and he sees reason for suspecting that it depends on some cause of irritation in a nerve-filament of the lower extremity. He seeks, in the history of the case, and by a physical examination, for this irritating cause, and discovers that the patient had, years ago, received a gunshot wound in the thigh. There is no evidence that the ball was ever extracted. A close examination seems to confirm the conjecture that it still lies imbedded in the muscles of the thigh. An exploratory operation is performed and the ball is actually found lying upon a branch of the sciatic nerve. It is removed and the patient has no more convulsions.[1]

In this case the physician's knowledge of pathology enabled him to discriminate between the varieties of convulsion as to their proximate causes; his acquaintance with physiology familiarized him with the phenomena of reflex-nervous action and enabled him to detect the seat of the irritation, and finally his dexterity in practical surgery placed it in his power to cure the patient of a dreadful malady. But, in this case from beginning to end, Therapeutics was not called into play.

Again, the patient, a child of two and a half years, a subject to epileptiform convulsions, which occur daily, often twice a day. They first appeared when the child was fourteen months old and soon after it was weaned. They are now producing a very sad effect upon the child's intelligence.

[1] This occurred in the practice of a distinguished surgeon of Massachusetts.

The same question of proximate cause, which the last case gave rise to, suggests itself first of all to the mind of the physician. Careful investigation satisfies him that there is probably a constant or a frequently repeated source of irritation in the digestive apparatus. What so probable cause of irritation in these organs as improper food? The question is put at once, "What food does your child take?" "Nothing whatever, except molasses-cake and milk." "How do you make molasses-cake?" "Three cups of flour, one of butter, one of milk, one of molasses, one egg and a teaspoonful of pearlash!" "Very bad for your child! Give it good wheat bread and milk and nothing else, except this powder" (of sugar of milk as a placebo). The diet of the child is thus reformed, but no medicine is administered, and the case is carefully watched. For one week the spasms steadily decrease in frequency and in violence, at the end of that period they cease and they never recur.[1] Thus an abnormal state of things, which would certainly have had a fatal issue, is rectified by the application of a knowledge of Physiology, Pathology and Hygiene; but Therapeutics has no part in it.

Errors in diet being perhaps the most numerous of all of which the community are guilty, instances like this might be accumulated to an indefinite extent. It is clear that a large part of a medical man's practice is made up of similar cases, and consequently that a great many serious cases may be successfully treated without recourse to medicine—without the aid of *Therapeutics.*

Again, a patient presents herself with the following history: Within three months she has rapidly emaciated; her sleep is disturbed, her appetite gone; she has night-sweats; is prone to diarrhœa; takes frequent deep inspirations, which fatigue her; has a dry, hacking cough; complains of extreme weakness and despondency. A careful physical examination fails to discover organic disease of thoracic or abdominal organs,

[1] This case occurred in 1857. There has been no recurrence of spasms. The child on good diet is healthy.

nor does an investigation of her domestic or social relations disclose any source of mental or affectional anxiety. The case is perplexing as to its inducing cause, and the physician inclines to ascribe it to dynamic causes and to have recourse primarily to specific remedies for its cure,—in other words, to call Therapeutics to his aid,—when he discovers that the patient is nursing an infant of twelve months. Her milk is very abundant, but a microscopic examination shows that it is extremely deficient in oily matter. It is now clear that the patient is unequal to the drain to which nursing subjects her; she is suffering from consequent anæmia. It is ordered that she wean her babe at once. As soon as she does so, appetite returns, the cough and night-sweats disappear, and strength and flesh are restored. Now, it is true that, in such a case, the decline of vigor may be *retarded*, and after weaning, the restoration may be *accelerated* by a judicious application of Therapeutics; but it is unquestionably true that Therapeutics would play a very subordinate part, since no administration of medicines could be an offset to the drain which is exhausting the patient.

In cases of similar exhaustion, facial Neuralgia of a regularly intermitting character sometimes occurs and bids defiance to Therapeutics, the patient meanwhile declining rapidly in strength and health until, medicine being perhaps altogether discarded, but the patient induced to wean her infant and thereby enabled to take hearty food, strength and flesh return, and the neuralgic pains cease. In this case, also, the restoration may be *accelerated* by a judicious recourse to Therapeutics, but here too Therapeutics must be subordinate to Hygiene.

Once more, a patient, who exhibits signs of depraved nutrition, complains of a burning pain under the angle of the right scapula, with a tight aching across the back between the scapulæ. I fear that repeated prescriptions will fail to relieve this burning pain unless the physician's knowledge of pathology shall have suggested to him that the symptoms

result ultimately from a too steady use of the needle with too rapid a motion, and unless his skill in Hygiene shall have enabled him so to order her mode of life as to combine due recreation and exercise in the open air with her necessary labor. Here, as in other cases, Therapeutics may of course come in and play a subordinate part.

Finally, a patient has severe darting and aching in the shin, especially at evening, with coldness of the extremity, and, after the pain had lasted an hour, great sensibility to touch, relieved by warmth and by continued motion. This case has been treated by a variety of Therapeutic agents in the hands of several learned practitioners, but with no success. It is noticed that the patient, in the course of his business, incurs great exposure to dampness and cold. A woollen stocking is advised, and he enjoys ever after adopting it almost complete freedom from suffering, and which is rendered absolutely complete by a few doses of Rhus toxicodendron. It must be remarked that this remedy had been previously administered in every variety of potency and dose.

Instances of a similar character to these might be indefinitely multiplied. They go to show, not that medicines are vain or unnecessary, but that very many cases of severe suffering and even of what threatens to become fatal disease occur in general practice, which call for and are successfully met by the application of scientific knowledge apart from Therapeutics. They show that an extensive and thorough knowledge of Physiology, Pathology and Hygiene are indispensable to the physician to enable him to make that preliminary analysis of a case by which he will determine under what category the case belongs, and whether or not it calls for treatment by medicines in part or entirely—whether it comes under the domain of Therapeutics or not. They show, moreover, that, inasmuch as Homœopathy directly involves the science of Therapeutics alone, there is a large field which is occupied and cultivated in common by Homœopathists and practitioners of the Old School. They serve in part, also,

to mark the boundaries of that field in which "Rational Medicine" may legitimately claim dominion, in which her chief honors have been won, and in which the great advances which she boasts of having made in the treatment of diseases within the last thirty years have almost exclusively been achieved,—the field of Hygiene. It is fitting that we study for a few moments this territory of Hygiene, which is common to practitioners of all the varieties of Therapeutic faith and practice,—this science on which, together with the science of Therapeutics, the whole art of medicine is based—its subjects, limits, conditions, and mode of growth.

THE NATURE AND LIMITS OF THE SCIENCES OF HYGIENE AND THERAPEUTICS.—The living organism possesses a susceptibility to the action of certain general stimuli, such as light, heat, electricity, aliment, atmospheric air, etc. The action and reaction of these stimuli and this susceptibility are the conditions of life. So long as they act upon it in a due relative proportion, as regards intensity and quantity, the equilibrium of the functions is preserved and the organism continues in healthy action. The absolute withdrawal of one of the stimuli for any considerable length of time results in death. A disturbance of their due proportion, in respect of intensity or quantity, produces an abnormal performance of function in the organism—a deviation from health—disease. But these stimuli are continually varying in proportion, or, in other words, the relative susceptibility of the organism is continually changing. Why does not disease constantly exist? Because the organism is endowed with either a faculty of provisionally supplementing to a limited extent one stimulus by another, or with a kind of elasticity,—a power of enduring for a certain time a disturbance of the equilibrium of these stimuli, and of rebounding to a normal performance of functions again so soon as the natural proportion of the stimuli is restored or the deficiency made up. In this respect, the living organism

differs from an inorganic machine, which cannot, in the nature of things, possess any power to endure a disturbance of that equilibrium of forces which is the condition of its normal working without a disorganization from which it has no inherent power to recover.

But, in the organism this elasticity has its limits. This "*vis medicatrix naturæ*" is not inexhaustible. If the due proportion of the stimuli remain too long disturbed, the functions of the organism become permanently deranged—at least, to such an extent, that no restoration of the balance of the stimuli will cause a return to their normal performance. The functions are and remain deranged—disease has occurred; or, if we choose to call *every* deviation from a state of equilibrium disease, then we may say that now disease ensues which has no tendency to revert to health without the intervention of some extraneous influence foreign to the organism and different from the general stimuli aforesaid. Since, then, the general stimuli will not bring back the organism to a healthy action, a new element must be sought for and introduced, the action of which upon the susceptibilities of the organism may cause a restoration to health. This new element will be a *special* stimulus. Being foreign to the organism and different from the general stimuli, not only must it act upon susceptibilities in the organism which the latter do not awaken, but the formula which shall express its relations to those susceptibilities, and which shall furnish the rule for its employment, can never be discovered by a study of Physiology, for Physiology concerns herself with the relations of the *general* stimuli aforesaid and the *general* susceptibilities of the organism. This formula of the relations of *special* stimuli and *special* susceptibilities can be discovered only by the application of induction to a multitude of instances of the action and reaction of *such stimuli* and *susceptibilities*, and confirmed by subsequent deductive verifications. This formula will constitute an empirical law, which will be the law

or fundamental principle of *Therapeutics.* For the application of *special* stimuli to the *diseased* organism is the domain of the *science of Therapeutics*, while all that concerns the *restoration and maintenance of a proper equilibrium of the general stimuli* appertains to *the science of Hygiene.*

These propositions may be more intelligible if illustrated by a reference to daily experience. A healthy man is exposed to an unusual degree of cold; in other words, there exists for him a deficit of heat—one of the *general stimuli* which are necessary to maintain life. Nature has anticipated variations in the supply of this stimulus from external sources by her liberal provisions of calorific apparatus within the organism. Despite the operation of this provision, he is chilled and suffers from rigors, etc. After a time he seeks shelter, sits by a fire, takes a warm drink; in other words, receives from external sources an excess of that general stimulus from deficiency of which he has been suffering. His functions resume their normal play. He is in perfect health. Here the equilibrium of the functions has been disturbed and (if we use terms with extreme rigor) disease has been produced, but not to a degree deyond the provisions of the *vis medicatrix naturæ*—the natural tendency to a restoration of the balance of the functions. The case was treated upon "general principles" in accordance with the maxim *causa sublata tollitur effectus.* And this maxim represents, in fact, the great law of Hygiene, viz.: that it be ascertained what stimulus has been deficient or excessive in quantity or abnormal in quality, and that the equilibrium of the stimuli be restored.

But let us suppose that the same man has again been exposed to cold, perhaps to a greater degree. He seeks shelter and essays to restore the lost heat, but without avail. Despite the fire and warm drinks, the rigors continue and are succeeded by fever and quickened respiration, cough, etc., or by rheumatic pains, redness and swelling, etc. Why this difference between the cases? This case too has been

treated on "general principles." The cause has been removed, why has not the effect ceased? The equilibrium of the general stimuli has been restored and the loss made good; why is not the normal equilibrium of the functions re-established?

The normal proportion of the general stimuli, it is true, has been restored, but during the disturbance a *new element* had been introduced into the problem. The organism had suffered a dynamic and then an organic change. The functions are permanently modified. The general stimuli may henceforth be balanced never so carefully, and in strictest accordance with the rules of Hygiene; the organism will not respond.

Its functions are performed after a new fashion. The organs are not susceptible to the wonted stimuli applied according to the laws of Hygiene. The organism has passed from a state of health into one of permanent disease. The general stimuli which, modified and balanced under the laws of Hygiene, sufficed to steady it as it rocked and swayed in its rapid course along the rough and crooked railway of life, will no longer answer the purpose, for in its rocking it has run off the track, and is now bumping along over the cross-ties, making headway, it is true, but toward its own destruction. It needs now the intervention of some new agent acting under a new law—of a jack-screw and levers operated by forces from without—to reinstate it on the road of healthy action. The wonted general stimuli under the laws of Hygiene being insufficient, *new* stimuli of a *special* character must be applied according to a *new* law. This new law, whatever it be, is the Therapeutic law, and these new stimuli are Therapeutic agents, and the study of the law and of the agents constitutes the science of Therapeutics.

Having thus marked out its *limits*, we have next to inquire what the *nature* of any possible science of Therapeutics must be. Its subject is the modified functions and organs of the body. Its agents are *special stimuli* drawn

from whatever region of the external world. By what sort of a formula can these agents be applied to that subject? Can the Therapeutist act on "general principles" as the Hygienist does? Can he act on the maxim *causa sublata tollitur effectus?* Obviously he cannot. In so far as the cause of disease can be discovered in external influences, the treatment falls within the limits of the science of Hygiene as already discussed. In so far, however, as the cause of disease is identical with the essential cause of the modification of function or organ which we recognize as the disease, it can never be discovered, for it is the same, in its nature, as the cause of healthy functional or organic action; in other words, it is life itself, the nature of which, as of every first cause, is inscrutable. It being impossible then to ascertain the essential cause of disease, and to apply a remedy according to the *rational* method—as the Hygienist does—the Therapeutist is necessarily thrown back from an attempt to investigate *first causes*, to the study of *phenomena* and to the adoption of the *empirical* method. In accordance with this method, the subject of his researches will be respectively the phenomena manifested by the patient, and the phenomena produced by the special stimulus, and his endeavor must be to discover a general formula which shall express a constant relation between these two series of phenomena and shall serve as the Therapeutic law. In thus acting, and upon this method, he will do precisely what the student of every branch of Natural Science does and has done. For in his inability to find out the essential cause of the phenomena that form the subject of his study, the physician finds himself in the very same predicament as the Naturalist who likewise has vainly sought to discover the essential causes of the phenomena of gravitation, of light, of chemical action, and of electricity. As the physician is unable to discover the essential nature of life and organism, whether normal or modified, to ascertain the cause of health or of disease, and is therefore unable to treat disease upon the principle "tolle causam"

(except in matters of Hygiene, as before stated), or "on general principles;" so the Naturalist has been compelled to abandon the rational method, such as Aristotle proposed and philosophers elaborated up to the time of Bacon and Newton, and to adopt an empirical method in which the fundamental principle is an empirical law or generalization expressing the relation between two series of phenomena. The science of Physics, for example, consists of the phenomena respectively of two bodies, or series of bodies, so far as density and volume are concerned, and of the law of gravitation which expresses the relation between these respective phenomena.

The Therapeutist, then, abandoning all idea of constructing a science of Therapeutics on the rational method, must have recourse to the empirical, as the elaborators of other natural sciences have done. The elements of his science will be as follows: He has to deal with a subject known by its phenomena — the sick body; with an agent known also by its phenomena—the drug; and with a law which shall tell how to apply the agent to the subject for the accomplishment of a cure,—a law which shall express the general relation between the drug and the morbid organism.

The following tabular statement will show more clearly the nature of Therapeutics as a science, and its harmony with other Natural Sciences; for every Inductive Natural Science (except those of classification) consists *elementarily of two series of independent phenomena, connected by the formula of their general relation:*

THERAPEUTICS.

Morbid Functions & Organs, Or, Pathology and Pathological Anatomy, Or, Sick-Phenomena.	Therapeutic Law.	Toxic Functions & Organs, Or, Pathogenesis and Pathogenetic Anatomy, Or, Drug-Phenomena.

PHYSICS.

Phenomena of the Sun, as regards Volume and Density.	Law of Attraction.	Phenomena of the Earth as regards Volume and Density.

CHEMISTRY.

Properties of Potassa.	Law of Chemical Affinity and Definite Proportion.	Properties of Sulphuric acid.

OPTICS.

Properties of the Luminous Body.	Law of the Diffusion of Light.	Properties of the light-receiving body.

Though thus simple in theory, Therapeutics is in reality the most complex of all the natural sciences. Each of the classes of phenomena requires for its study the aid of several auxiliary sciences. Thus, in order to know thoroughly the phenomena of disease, we must call to our aid Anatomy and Physiology, Chemistry, Physics and Psychology. To know and understand thoroughly the phenomena of artificial disease or pathogenesy, we must avail ourselves of the same auxiliary sciences. The more imperfect our knowledge of these sciences, the less complete will be our acquaintance respectively with the phenomena of Pathology and Pathogenesy, and the converse. But, however complete or partial may be our knowledge of these two classes of phenomena, the relation between them, as known, remains the same, and is always expressed by the therapeutic law. The same is true of Physics and of all the natural sciences. Our knowledge of the physical properties of matter is continually increasing. The more complete it is, the more exact will be our application of the law of attraction. But complete or incomplete, the law is equally applicable, and *pro tanto* available.

THE CONDITIONS OF A SCIENCE OF THERAPEUTICS. — We come now to consider two conditions to which every natural science must be subject, and which may therefore serve as tests of its fitness to be regarded as a "Science." The first of these is a capability of infinite progress in each of its elements without detriment to its integrity as a whole.

We have already seen that the science of Physics is capable of such progress in the phenomena with which it deals. Our knowledge of these phenomena is continually growing more comprehensive and more minute, and new elements of knowledge are continually coming into our possession with regard to them; yet their relation to each other through the law of attraction remains the same, and the science, in its nature and structure, receives no modification. But not only are its subjects, the phenomena, capable of progress; the law itself may advance from a minor to a grander generalization, as it was advanced, step by step, by Newton, and still without destruction of the previous steps. The same is true of Chemistry. As our means of investigation become more exact and extensive, our knowledge of the properties of bodies increases; yet the law of Chemical Affinity in definite proportions remains undisturbed. Furthermore, this law itself becomes the subject of more extended generalizations. The law as established by Black and Cavendish was suspended, yet not overthrown (nor the science disturbed) by the wider generalization of Dalton's Atomic Theory; and this again forms but a part of Faraday's Theory of the Identity of Chemical Affinity and Electrical Attraction, and amid all these revolutions in abstract theory, the science of practical Chemistry has held an even way.

In the science of Optics, again, the phenomena of the luminous body and those of the light-receiving body are connected by the law of the transmission and diffusion of light. This structure of the science and all that is based upon it rests undisturbed through all the mighty changes which recent discoveries have wrought in our knowledge of Optics — enriched but not revolutionized by them. But while the progress of the science has been thus rapid and uninterrupted, philosophers have been and remain at variance respecting the very nature of light and its mode of propagation. One school holds to the theory of emission, another to the undulatory theory. Now, let us suppose

that the science of Optics, instead of being based upon an empirical law, the result of induction, and which expresses the relation between two series of observed phenomena, had been based upon *a theory of the essential nature of light* and its mode of transmission. It is very evident that the whole structure that might be built upon the theory of emission would be toppled over so soon as the theory of undulation should be established. And should subsequent observation again lend countenance to the theory of emission, down would go, of necessity, the whole science as built upon the undulatory theory. And thus, inasmuch as experience and sound philosophy render it tolerably certain that we shall never attain absolute knowledge on this subject, we should have, through all time, an endless succession of half-completed fabrics and hopeless ruins, but no infinitely progressive science of Optics.

This furnishes an exact illustration of what would be the history of any science of Therapeutics not constructed upon the empirical plan already elaborated. Let us suppose that instead of taking as the subject of action, the *phenomena* of *disease* as derived from observation with all the aid which auxiliary sciences can afford, we should take a theory of disease,—its *essential nature and mode of development*,—and upon this theory should construct a science. All would go well, until facts should show that our theory was insufficient and untenable (and facts would show this, since, as we have already seen, the essential nature of disease is inscrutable). Now our whole structure would be overthrown, nor would the fragments even be available for the construction of another science or another theory, for they would consist of observations made, not independently, but on the basis of a theory and with reference to it. There could thus be no possibility of steady, uninterrupted, infinite progress in the science. There would be a succession of dynasties of theory, each based on the overthrow of a former, and each in turn demolished by its successor. That

this is the history of the Therapeutics of the Old School of medicine up to the present day is admitted by common consent. It will continue until attempts to erect a rational science of Therapeutics upon a theory of disease, confounding the law with the phenomena, shall give place to the construction of an empirical inductive science.

"What then!" it may be asked, "shall we not investigate and theorize upon the nature of disease in general, or of diseases in particular?" By all means investigate and theorize, just as physicists do upon the nature, properties and transmission of light, but do it as studying the science of Pathology, not that of Therapeutics, and avoid most carefully making these theories, which can be at best no more than temporary aids to the observation and grouping of phenomena, the basis of a practical science. If, however, they throw light on the study of phenomena, render our observations of them more keen and more exact, or afford us connecting links between isolated groups of phenomena,—and they will do all this,—they will render valuable *indirect* aid to the science of Therapeutics, just as similar investigations of light have advanced our knowledge of Optics.

A second condition or test of a natural science, and therefore of any proposed science of Therapeutics, is that it shall provide for the prediction of future events within its own domain. It must furnish means of *prevision.* The problem must be as follows: Given the law and one series of phenomena, to state the corresponding series of phenomena on the other side. This condition is admirably stated by Whewell: "Men cannot help believing that the laws laid down by discoverers must be in a great measure identical with the real laws of nature, when the discoverers thus determine effects beforehand, in the same manner in which nature herself determines them when the occasion occurs. Those who can do this must to a great extent have detected nature's secret—must have fixed upon the conditions to which she attends, and must have seized the rules by which

she applies them. Such a coincidence of untried facts with speculative assertions cannot be the work of chance, but implies some large portion of truth in the principles on which the reasoning is founded. To trace order and law in that which has been observed, may be considered as interpreting what nature has written down for us and will commonly prove that we understand her alphabet. *But to predict what has not been observed* is to attempt ourselves to use the legislative phrases of nature; and when she responds plainly and precisely to that which we thus utter, we cannot but suppose that we have in a great measure made ourselves masters of the meaning and structure of her language. The *prediction of results* even of the same kind as those which have been observed, in *new cases*, is a proof of real success in our inductive processes."[1]

The discovery of the planet Neptune by Leverrier and Adams affords an instance of this prevision, in Astronomy, the details of which are familiar to all. The problem was: Given the law of attraction and a series of phenomena, consisting of certain unexplained perturbations of Jupiter—to find the other and corresponding series of phenomena. The calculations were made; the resulting phenomena were stated to be those of an undiscovered planet of a given size and orbit and a definite location, the existence of which was accordingly affirmed and its discovery predicted. Physicial investigation confirmed the induction.

This condition applies to all natural sciences, and of course to Therapeutics. The problem would be: Given the Therapeutic law and a certain series of phenomena of natural or drug disease, to find the corresponding series of phenomena of drug or natural disease.

DOES "RATIONAL MEDICINE" FULFIL THESE CONDITIONS?—This being the nature and these the conditions and tests of

[1] Whewell, *Philosophy of the Inductive Sciences*, vol. ii., pp. 64, 65.

the only possible science of Therapeutics, we come now to inquire whether the Old-School Therapeutics are of this nature or will sustain these tests. The works of the foremost men of the Old School abound in admissions of their lack of a therapeutic law and of the chaotic state of their practice.[1] But the efforts of these men to remedy this sad state of things show a constant misapprehension of the philosophy of the science they are striving to construct.

We may divide Old-School Therapeutics into two methods. The one bases the plan of cure upon a theory of the nature of the disease,[2] endeavors so to study the pathology of the disease as to form a sound hypothesis of its *modus operandi* and then essays the cure upon "general principles." It undertakes, in fact, to act in Therapeutics upon what we have seen to be the true method in hygiene. There are three objections to this method, any of which would be conclusive were there not a prior difficulty in the *simple impossibility* of arriving at a knowledge of the nature of the disease, which is *modified life*. A simple example will show this impossibility. In Pneumonia the blood contains an excess of fibrine. How happens this? Why, either the elimination of fibrine has been retarded or its fabrication has been accelerated. What are the agents of these respective processes? Cells—which are essentially similar in all parts of the body—and yet whose structureless walls possess the power of separating from the circumambient fluids the most diverse substances in different organs—nay, even of manufacturing new substances. Now, until we can learn the secret of this mysterious power of the cell wall, which begins and ends with life, which we can neither comprehend nor imitate nor simulate, we shall not arrive at an understanding of the *nature* of any disease.

[1] See Forbes, Bennett, Tod, Holmes and others.

[2] Thus Prof. Geo. T. Elliott in his recent Valedictory to the Graduating class at Bellevue College Hospital says: "In these wards, by those bedsides, have you seen the patient application of the searching analytical laws by which we seek to discover the essentiality of disease."

Of the other objections to this method the first two are somewhat paradoxical. The method constitutes rather a congeries of sciences of Therapeutics, based on theories of isolated groups or types of disease, than a single, all-embracing science founded on one comprehensive theory of disease. Hence, a new science must be formed for every new type. It is too *special*. On the other hand, it is too *general* to embrace all *the phenomena of each individual case*. For observation shows incontestably that so great are the varieties of temperament and relative organization and condition among men, that no two cases of the same disease, so to speak, are exactly alike.

Such a Therapeutics must be necessarily based on a theory which is a generalization, and which therefore overlooks those phenomena of a given case which are peculiar to the individual patient and takes cognizance only of those phenomena which are common to a great number of cases. Now, a method which requires such generalization and makes no provision for the strictest individualization is radically vicious.

The eye of science regards natural phenomena with the most absolute impartiality. In her view there are no *trifles*, no events, subjective or objective, which are "irrelevant" and "of no moment." A method of Therapeutics, however, which selects a few symptoms, calls them pathognomonic of Pneumonia, constructs upon them a theory of the disease, and from this deduces a mode of cure, must disregard many phenomena of every individual case. It arbitrarily pronounces them "accidental"—but Nature knows no *accidents*.

So far is this method from meeting the requirements of inductive science, that it is not too much to say that this process of generalization actually leads to the treatment of cases which have no real existence and never had any,—in fact, to the *treatment of abstractions*. For who ever saw a case in which none but the pathognomonic symptoms of Pneumonia were present—or of Rheumatism or Dysentery?

Who ever saw ideal cases of these diseases? We all see cases which present the pathognomonic symptoms *plus certain others* peculiar to the individual, but these vary with the individual, and thus make each case different from every other. We see and treat the Pneumonia or Rheumatism of John Doe or Richard Roe, each of which presents, in addition to the pathognomonic signs of the disease, certain peculiar modifications impressed upon these signs by the temperament and idiosyncrasies of these persons, along with other symptoms peculiar to each. For such an individualization of disease as would give equal (or, as they deserve, higher) rank to these peculiarities of the individual patient, the method under consideration makes no provision. This fallacy was perceived and well exposed by the Leipsic Professor who recently exclaimed to his class: "Gentlemen, we have to do with *patients* and not with *diseases*."

The next objection to this method is that it does not fulfill the first condition of a physical science. It is not capable of infinite progress. This point needs no elaboration. It was fully considered when the condition was stated. In confirmation of what was then said, I need not refer to the successive rise and decline of Brunonism, Broussaisism, the humoral and solid pathology, the theory of the dyscrasias, and the reign and decadence of Iron, Iodine, Cod Liver Oil, etc.

The second method of Old-School Therapeutics is the methodical, which discards theory and founds its rules for treating disease upon numerical data, obtained from observation or by experience upon the sick.

Given records of three series of cases, 1000 in each, and under three modes of treatment: Under treatment A, 500 recovered; under B, 300; under C, 200. A is adopted as the established mode of treatment for the disease—"*Risum teneatis, Amici?*" An elaborate criticism of this method is unnecessary. It is enough to say that it does not fulfill either condition of a physical science. It is incapable of

infinite progress. The addition of 500 cases to the 3000 above named might modify all conclusions and place treatment C in advance of A or B. There would result a complete revolution in practice and in the direction of investigation.

But it utterly fails to enable us to foresee and provide against new forms of disease. If a thousand or two of cases must be seen and experimented upon, in all conceivable ways, before definite and trustworthy conclusions as to the best treatment can be arrived at, who would not pray that his turn might come among the third or fourth thousand? Who would not pity the victims among the first thousands?

Notwithstanding these obvious objections, this method of constructing a science of Therapeutics upon observation, whether at random or based on experimentation upon the sick, has the sanction of some of the highest names in the profession—among which it is sufficient to name Louis and Forbes. It demands, therefore, a more extended consideration. As it involves the application of both Mathematics and Logic, the opinion of the highest authorities in these respective sciences should be conclusive as to the soundness of the method, and we content ourselves with quoting two of them:

M. Auguste Comte, one of the first living mathematicians, and who would therefore be disposed to apply mathematical methods to the natural sciences wherever this were possible, expresses himself in the following manner concerning the numerical method in medicine:

"Indeed, the spirit of calculation tends in our day to introduce itself into this study (Physiology), especially into that part of it which relates to medical questions, by a far less direct method, under a far more deceptive form, and with infinitely more humble pretensions. I wish to speak of that assumed application of it which is called the Statistics of Medicine, from which so many *savants* expect great things, and which, from its very nature, can lead only to profound

and direct degradation of the medical art (which would be reduced by it to a method of blind enumeration). Such a method, if we may be allowed to call it by the name of method at all, cannot in reality *be anything else than absolute empiricism,* disguised under the frivolous garb of mathematics. Pushed to its extreme logical consequences, it will tend to make all rational medication radically disappear from medicine, by conducting the practitioner to make random trials of certain therapeutic measures with the object of noting down, with minute precision, the numerical results of their application. It is evident, on principle, that the continued variations to which all organism is subject, are necessarily more pronounced in a pathological than in a normal state, and as a consequence of this fact, the cases must be even less exactly similar, whence results the manifest impossibility of making a judicious comparison between two curative methods derived from data, furnished by statistical tables alone, independent of some sound medical theory. No doubt, some direct experimentation, restrained under proper limits, might be of great importance to medicine as well as to Physiology, but it is precisely under the strict condition that it shall never be merely empirical, but shall always attach itself either in institution or in its interpretation to an entire system of corresponding positive doctrines. Notwithstanding the imposing aspect of the forms of exactness, it would be difficult to conceive of an opinion in Therapeutics more superficial and more uncertain than that which rests solely on the easy computation of fatal and favorable cases, to say nothing of the pernicious practical consequences of such a manner of proceeding, when one could not beforehand exclude any kind of attempt.

"It is really deplorable that geometricians have sometimes honored with some kind of encouragement, such a profoundly irrational abberration by making vain and puerile efforts to determine by their illusory theories of chances, the number of cases sufficient to make these statistical results legitimate."[1]

[1] *Cours de Philosophie Positive,* par M. Auguste Comte, tom. iii., pp. 418–420.

And with a direct reference to the method of obtaining a correct system of Therapeutics by experimenting with individual medicines and individual cases of diseases, and forming of the results statistical tables from which deductions are to be drawn by the numerical method, the highest modern authority in philosophy, John Stuart Mill, speaks in his *System of Logic*:[1] "Let the subject of inquiry be the conditions of health and disease in the human body; or (for greater simplicity) the conditions of recovery from a given disease; and in order to narrow the question still more, let it be limited, in the first instance, to this one inquiry: Is or is not a particular medicament (Mercury, for instance) a remedy for that disease?

"The experimental method would simply administer Mercury in as many cases as possible, noting the age, sex, temperament and other peculiarities of bodily constitution, the particular form and variety of the disease, the particular stage of its progress, etc., remarking in which of these cases it produced a salutary effect, and with what circumstances it was on those occasions combined. * * * *

"When we devise an experiment to ascertain the effect of a given agent, there are certain precautions which we never, if we can help it, omit. In the first place, we introduce the agent into the midst of a set of circumstances, which we have exactly ascertained. It need hardly be remarked how far this condition is from being realized in any case connected with the phenomena of life; how far we are from knowing what are all the circumstances which pre-exist in any instance in which Mercury is administered to a living being. This difficulty, however, though insuperable in most cases, may not be so in all; there are sometimes (though I should think never in Physiology) concurrences of many causes in which we yet know accurately what the causes are. But when we have got rid of this obstacle, we encounter another still more serious. In other cases, when we intend to try an experiment, we do not reckon it enough that there be no cir-

[1] Harper's edition, 1848, p. 260.

cumstances in the case, the presence of which is unknown to us. We require also that none of the circumstances which we do not know of shall have effects susceptible of being confounded with those of the agent whose properties we wish to study. We take the utmost pains to exclude all causes capable of composition with the given cause; or if forced to let in any such causes we take care to make them such that we can compute and allow for their influence, so that the effect of the given cause may, after the subduction of those other effects, be apparent as a residual phenomenon.

"These precautions are inapplicable to such cases as we are now considering. The Mercury of our experiment being tried with an unknown multitude (or let it be a known multitude) of other influencing circumstances, the mere fact of there being influencing circumstances implies that they disguise the effect of the Mercury, and preclude us from knowing whether it has any effect or no. * * * *

"In phenomena so complicated it is questionable if *two cases similar in all respects but one ever occurred;* and were they to occur we could not possibly know that they were so exactly similar.

"Anything like a scientific use of the method of experiment in these complicated cases is therefore out of the question. We can, in the most favorable cases, only discover, by a succession of trials, that a certain cause is very often followed by a certain effect."

HAS "RATIONAL MEDICINE" ACCOMPLISHED NOTHING?—But it may reasonably be asked, can it be possible that a large number of men of every generation since Hippocrates, and among them some of the brightest intellects the world has known, should have been engaged in the study and practice of medicine, with no better result than this—a simple vacuity? I admit the rare intelligence and devotion of the laborers. But, in the first place, the task is mightier than any other essayed by man; may it not reasonably require ages

to complete its plan? Again, labor, however intelligent and devoted, if misdirected, must fail of its end. If one generation of intellectual giants erect a towering fabric and their equally lusty successors demolish it, what does the third inherit save a heap of rubbish which it may require the best years of its life to clear away. Generation after generation of wise men labored to construct the physical theory of the universe, yet not until the recent day of Newton did we learn how to *begin* aright. True, Newton's predecessors from the earliest ages observed and stored away a mass of isolated facts of the greatest value, but they knew not the magic word wherewith to charm them into order. In like manner, it is not denied that we are indebted to our predecessors for a vast number of isolated facts of incontestable value as materials to be used in the construction of a science of Therapeutics. We are indebted to them also for the elaboration of those subsidiary sciences—Anatomy, Physiology, Pathology, Chemistry and the like—in which, indeed, the great glories of the medicine of to-day have been won, and without which, we cheerfully admit, Therapeutics as a science could not exist.

But it is said that Therapeutics *must* have advanced inasmuch as the rates of mortality have steadily diminished. *One* sort of improvement we gladly acknowledge. Simultaneously with the spread of Homœopathy, Old-School physicians began to learn to abstain from mischievous modes of treatment formerly pursued, and which terribly enhanced the natural fatality of diseases. Professor Bennett, in the introduction to his work on Clinical Medicine, specifies Apoplexy, Pneumonia and Pleurisy, Syphilis, Small-pox, Phthisis, Bright's disease, and Favus, as diseases in treating which, he says, "great improvements have been made;" and he adduces this as a proof of the advancement of medicine. A reference to the chapters in which he treats of these diseases shows that he conceives the "improvement in their treatment" to consist in abstinence from methods

which were formerly employed and which he clearly proves were very mischievous. For example, Bloodletting in Apoplexy, Pneumonia, Pleurisy, Phthisis, and Bright's disease —the immoderate use of Mercury in Syphilis—heroic medication and external warmth in Small-pox, and avoidance of all internal medication for Favus, which he conceives to be a vegetable parasite. This improvement, then, is purely negative, a very poor ground on which to claim *positive* advancement in the construction of a science, however deserving of the gratitude of suffering humanity. The formula for improvement of all kinds is, "Cease to do evil; learn to do well." We admit that the Old School are learning to obey the former, the negative clause. We invite them to advance and join us in the latter, the positive.

Again, an extended knowledge of the remote causes of disease and of the principles of Hygiene has enabled medical men to prevent the occurrence or the spread of many forms of disease. And far be it from us to undervalue, through any love of a newly constructed science of Therapeutics, this branch of a physician's function, the forestalling or arrest of maladies by Hygienic means. As increased knowledge in this department of science has enabled us to dispense with therapeutic agents in many cases in which they were formerly resorted to, so we firmly believe that, with the advancement of learning, Hygiene will more and more curtail the boundaries of Therapeutics, until the day shall come when the physician shall be prized as the *preserver* rather than solely the *restorer* of health, and shall be consulted respecting the means of *preventing* rather than solely of *curing* disease, and the "family doctor" shall be regarded as the confidential adviser, valuable in proportion as he *keeps* his client *out* of bed, just as the family lawyer is confided in, for the purpose of keeping his clients out of court! In that day there will be fewer apothecaries.

Does Homœopathy fulfill the Conditions of a Science of Therapeutics?—Returning now to our argument,

we find that the field is open for a science of Therapeutics. In the light of what has been said we proceed to examine the claims of Homœopathy to the honor of being that science.

In its structure as a science, Homœopathy conforms to the model we have delineated. It consists of a law or formula which expresses the relation between two series of phenomena, those of a given case of disease on the one hand and those of a given drug-proving on the other. The elaboration of each of these series is the province of various subsidiary sciences, and they are analogous in their mode of elaboration. Each series, however, is entirely independent of the other. Each may be pursued independently, as a branch of Natural Science and under the heads of Pathology and Pathogenesy respectively, researches may be made in each without any view to a practical application in the cure of the sick. It is only when connected by the law of their relation (the formula of similarities) that they constitute the science of Therapeutics.

Their application, moreover, in obedience to this law is based upon no hypothesis respecting the essential nature of either variety of phenomena or of their *modus operandi* where brought into operation. This may surprise some who know how earnestly Hahnemann argued on these very points in his *Organon*. But these arguments were no essential parts of his system. They were the results of an endeavor to commend his discovery to the prevalent way of thinking. They constitute the only controvertible part of his writings, and are the only positions of his which have not triumphantly withstood the assaults of his critics.

Coming now to apply to Homœopathy, as tests, the conditions to which we have shown that every inductive science must conform, we find in the first place that it is capable of infinite progress in each of its elements, without such progress involving the destruction or denial of what has been previously constructed or received. The study of the phe-

nomena (whether of disease or of drug-action) was limited at first to the observation of external manifestations and subjective sensations, as these might present themselves to our senses unassisted by any of the aids by which modern science has sharpened them, or to our minds unaided by that knowledge of the connection and mutual relations and dependences of symptoms for which we are indebted to modern discoveries in Chemistry and Pathology. But these advances in Pathology, great as they have been, have not altered the relation which the phenomena of natural disease bear to those of drug disease. These phenomena respectively, whether rudely apprehended, or clearly and fully understood in all their relations and interdependences, still bear the same relation to each other—expressed by the law *Similia Similibus Curantur*. And we can imagine no possible development of the sciences of Pathology and Pathogenesy which could alter this relation.

And then the law itself may be but a stepping-stone to a still wider generalization which shall one day embrace both it and something besides, and which shall make clear some things which we now see darkly. But should this occur, as the like has occurred in other Natural Sciences, there will be, there can be, no revolutionary action in it. It may be that the edifice, as we now occupy it, is still unfinished,—it may be that other stories are one day to be added,—but assuredly, as the tower is to the spire, as the buttress to the pinnacle, so will this generalization be to that which may be constructed upon it,—a basis,—an indispensable first step in the construction of the science.

The complete manner in which the second condition—that of *prevision*—is fulfilled by Homœopathy is a source of inexpressible benefit to the race. It follows from the very terms of the science, that if the phenomena of a given case of disease be known, the law of relation will at once point to the appropriate remedy (if this be contained in the Materia Medica); and this indication may be relied upon with

implicit confidence, even though no such case of disease has ever heretofore been subjected to treatment. Conversely, when the properties of a given drug have been investigated and its toxic phenomena well ascertained, the physician is able to pronounce with certainty what form of disease it will cure, even though no such disease has ever been witnessed or treated by himself, or by anybody. An illustrious example of this *prevision* was afforded by Hahnemann. The terrible fatality of Asiatic Cholera, on its first invasion of Europe, is well known. In extenuation of their lack of success, physicians of the Old School pleaded that the disease was new to them,—they had had no opportunities to study it, and to ascertain by experiment the effects of remedies upon it. The plea was plausible, but fatal to the pretensions of their science. In fact, it was good for nothing. For surely the first thousand cases should have afforded means enough for learning the Pathology of the disease and how to cure it, if *this were to be learned from Pathology.* But hundreds of thousands perished, and yet the percentage of mortality remained the same.

While the disease was still on the confines of Europe,—before it had invaded Germany,—long before either he or any of his disciples had ever seen a case of it, "Hahnemann, guided by the unerring therapeutic rule he had discovered, at once fixed upon the remedies which should prove specific for it, and caused directions to be printed and distributed over the country by thousands; so that on its actual invasion the Homœopathists and those who had received Hahnemann's directions were fully prepared for its treatment and prophylaxis; and thus there is no doubt many lives were saved and many victims rescued from the pestilence. On all sides statements were published testifying to the immense comparative success that had attended the employment of the means recommended by Hahnemann before he had seen or treated a single case. This one fact speaks more for Homœopathy, and the truth of the law of nature on which

the system is founded, than almost any other I could offer, viz.: that Hahnemann, from merely reading a description of one of the most appallingly rapid and fatal diseases, could confidently and dogmatically say, such and such a medicine will do good in this stage of the disease, such and such other medicines in that; and that the united testimony of hundreds of practitioners in all parts of Europe should bear practical testimony to the accuracy of Hahnemann's conclusions."[1]

We may add that in the second Epidemic of Cholera in 1849, the Old School, despite their experience in 1831–34, had but little better success, while again the justice of Hahnemann's conclusions and the claim of Homœopathy to that *prevision* which characterizes *a true science* were vindicated by the splendid success of the Homœopathic treatment.

John Stuart Mill, in the portion of his work on Logic from which we have already quoted, in speaking of the three methods of investigation,—that of observation, that of experimentation, and that of deduction,—after showing conclusively that the former two are inapplicable to medicine, speaks of the deductive method in terms which are (unintentionally of course, and for this very reason they are the more conclusive) a description of the philosophy of Homœopathy. "If, for instance, we try the experiments with Mercury on a person *in health*, in order to ascertain the general laws of its action upon the human body, and then reason from these laws to determine how it will act upon persons affected with a particular disease, *this* may be a *really effectual* method, but *this is deduction*."

How to Study the Science of Therapeutics.—The method of studying the two series of phenomena which, together with the law of relation, constitute the science of Therapeutics, follows from what has been said.

When first brought into the presence of a concrete case of disease, the business of the physician is to ascertain what

[1] Dudgeon's *Lectures on Homœopathy*, p. 37.

branch of medical science he is called upon to exercise. Is the case one which requires hygienic management, or therapeutic, or both, or is the patient beyond the reach of art? To answer these questions a diagnosis and prognosis must be made, and to make these, a knowledge of the remote and proximate causes and of the course and determination of diseases is required. In a word, a knowledge of Physiology and Pathology is indispensable on the very threshold of medical practice and before any question of Therapeutics has arisen.

When these preliminary questions have been settled and the case has been found to come within the domain of Therapeutics, its phenomena are to be studied in such a way that *all deviations from a normal state* may be perceived, as well those which are *common* to a *number of similar* cases, as more particularly those which seem to be peculiar to the individual case in hand, and which therefore serve to give it individuality and to distinguish it from all other and similar cases. The case is to be then individualized as sharply as possible, and a complete picture of the morbid phenomena obtained in their natural groups and connections.

Now, *morbid phenomena* are deviations from *healthy phenomena*. How can we recognize the *deviations* unless we are familiar with the *standard?* How can we appreciate *morbid phenomena* save through a knowledge of *Physiology*, which is the science of *healthy phenomena?*

In like manner we are able to get a complete picture of the morbid symptoms only by an orderly and methodical investigation; and such an investigation is possible to those alone who are familiar with the relations and sequences of morbid phenomena,—that is to say, with *Pathology*. A simple reference to practical experience will prove this. A patient complains of pain in her left hypochondrium, distress and faintness in the epigastrium, vertigo and various symptoms of dyspepsia, but never thinks of mentioning—perhaps is unconscious of—certain evidences of uterine disease, to which the attention of

the physician is instantly directed through his knowledge of the connection and sequence of symptoms. So, of the connection of certain forms of vomiting with disease of the brain or of the kidneys, etc., etc.

Clearly, then, Physiology and Pathology are quite indispensable to the physician, and they speak with little thought who affirm that these sciences are of no value to the Homœopathist and are disregarded by him. They are the sciences respectively of healthy and morbid phenomena. He cannot take the first step in the study of disease or of Materia Medica save by their aid. But he restricts them to their legitimate function. Pathology is for him *not a guide in Therapeutics*, but an *instrument which he uses in studying those phenomena which are to be respectively the subject and the agents of his therapeutic operations.*

Having, by the aid of Pathology, arrived at a complete and comprehensive knowledge of the morbid phenomena, he passes on beyond the confines of that science to a higher and more complex science, whose domain is the *relation* of the phenomena of which he has thus acquired a knowledge, with *other* phenomena. Through Pathology he learns to *know* disease, but it is through Therapeutics alone that he can *cure* it. And it is quite time that it were well understood not only by the profession but also by the public, that to know the nature and course of a disease is not of necessity to know how to cure it. It may be a necessary preliminary step—but it is nothing more. Nor is this true of medicine alone. My carriage breaks down; I well know where it is broken and why and how; yet this knowledge does not involve the knowledge how to forge and weld the iron that has broken and so to mend it. For that I require knowledge of another sort. The nature of Pneumonia, of Cholera, or Rheumatism is as well known as those of any disease can be; "their Pathology," as doctors say, "is well understood," yet this gives no clue to their therapeutic treatment,—it is no guide to the special stimulus which must be brought to bear on the

diseased organs to lead them back to healthy action. This stimulus must be discovered by quite another method; its discovery is the object of a distinct process.

Thus Pathology, restricted to its proper sphere, is an indispensable auxiliary to the study of the subject of Therapeutics. It may be further subservient in enabling the physician to group the symptoms of a case in such a way as more readily to marshal and retain them in memory. Nor is generalization of this kind at all repugnant to the letter or spirit of Hahnemann's method, or of homœopathic science.

The generalization to which Hahnemann objected was to that of disease in general upon nosological hypotheses made on theoretical grounds, and then applied *a priori* to individual cases. That to which we refer is a generalization made specially in each case, consisting of a grouping of connected symptoms under one general term and extending only to such pathological states as are well defined and constant, such, for example, as Anæmia, Plethora, the proportion between the affections of different parts of the nervous system, etc., under which we may group a number of *generic* symptoms, to the great relief of our memory, while at the same time the individual or characteristie symptoms are not only not obscured by the process but are even brought more sharply into view, as will be evident when we consider this matter more at length under the head of the Study of the Materia Medica.

The Study of the Materia Medica.—The method by which any subject may be most successfully studied must depend on the use which is to be made of the knowledge thus gained. It is proper, then, to inquire at the outset in what way the knowledge of Materia Medica is to be made subservient to the treatment of disease.

In accordance with the homœopathic law, we select for the cure of each individual case of disease that remedy of which the pathogenetic effects are *most similar* to the symptoms of

the case. *In the process of making this selection we must pass in mental review the various drugs which compose the* Materia Medica, take a comprehensive view of the pure effects of each, and institute a comparison between each in turn, and the case for which we are prescribing. This is the theory of the process.

Now, it is evident that, in order to select from a number of candidates one which most nearly resembles a given standard, we must be familiar, not merely with the general properties of all the candidates or of certain classes into which they may be divided, but also with certain properties more or less peculiar to each one of the candidates, and which shall serve to distinguish each of them from all the others. In fact our method requires the strictest individualization of both disease and remedy. We are so to study Materia Medica, as, above all, to bring into strong relief and fix firmly in memory those peculiarities of each drug which are not met with in any other, and which therefore serve to individualize and give *character to the drug* that produces them and which are called its "*characteristic symptoms.*" This term having been much and loosely used of late, it may not be unprofitable to devote a few words to the subject of characteristic symptoms.

By some writers the leading and most obvious and most frequently recurring symptoms are called characteristic. Thus Bennett calls fever a characteristic of the Exanthemata. By others the pathognomonic symptoms of a class of diseases are called characteristic,—by others the pathologico-anatomical.

Now, the signification of such a word as characteristic is not absolute. It depends on the connection in which you please to use it, and which is determined by the question, "*Characteristic of what?*" In the instances just adduced, the varieties of symptoms cited may indeed be called characteristic, but—characteristic of *what?* Of *classes* (the Exanthemata), of *groups* (nosological)—but not of *individuals.*

But the only sense in which Homœopathists can use the term is in its application to *individuals.* Hence a characteristic symptom must mean one which is possessed by none other than the individual drug of which it is predicated, and to which therefore it *gives character* as an *individual.* In this sense it corresponds precisely to those features of a man by which his friends are enabled to distinguish him from other persons and to recognize him at a glance.

It is obvious that these characteristic symptoms so precious to the Therapeutist may seem to be of little or no pathological value,—may even seem accidental to those who forget that there are no accidents in Nature. They would be valueless if we did not need to *individualize,* but could be content with *grouping* our diseases and remedies.

To the Naturalist whose object it is to *group* his specimens, it is sufficient to know that John Doe has a vertebral column, is a mammal, has two hands, and is a Caucasian—because this enables him at once to place John Doe in the variety Caucasian of the species man, and his analysis goes no farther. From this his whole physiological status follows. But these items of *general* knowledge would hardly enable the *sheriff* to recognize John Doe in Broadway. It is of no importance to the Naturalist that he has such "accidental" peculiarities as an aquiline nose, black eyes and hair, and a brown mole on the left ala nasi; but these very peculiarities are all-important to the sheriff, for they give him the means of detecting the object of his search upon the crowded street. It must not be forgotten, however, that the points on which the Naturalist laid stress are equally important to the sheriff; for if the latter should bear in mind only the *individual* peculiarities of the object of his quest and should *forget* that he is a Caucasian, he might find the former in the person of an Indian, or, if he should forgot that he is a bimanous creature, he might arrest a monkey.

To drop the figure, then, it is evident that we must seek to discover among the symptoms of every drug certain ones

that are produced by no other drug, and which shall serve *to distinguish it from all other drugs similar in other respects; that these symptoms will often be unimportant and* trivial in a physiological point of view; furthermore, that we must, for convenience sake, when the number of drugs in our Materia Medica has become considerable, endeavor so to group them, on the basis of certain clearly defined symptoms or collections of symptoms, that for the purpose of preliminary examination and comparison, these groups may be regarded and compared as though they were individual drugs.

Let us suppose a case of uterine hæmorrhage. As many as forty drugs probably produce uterine hæmorrhage. On the basis of this symptom, they form a group isolated from the three hundred and forty remaining drugs of the Materia Medica. We select this group from the Materia Medica, and now we must select a remedy from the group. It were a tedious task to consider and compare them one by one. But we group them again; ten of them produce dark-colored and ten florid hæmorrhage; ten a limpid and ten a clotted discharge. Our case has a dark-colored discharge. Our choice is now restricted to ten drugs. But of the ten which produce a dark discharge, only five produce simultaneously a congestive headache. Thus we are limited to five drugs. Thus far, the distinctions on which our grouping has been based (or which have been characteristic of the *groups*) have had a pathological significance and importance. We can find no such basis for any further subdivision into groups. But we observe in the case a peculiar *subjective* symptom. The patient complains "as though a living body were moving through the abdomen." This may seem trivial. It is equally, however, a symptom produced by Crocus, which is one of the five remedies to which our choice had been restricted, and it is produced by no other drug in the Materia Medica. It is, then, a *characteristic* symptom of Crocus, enabling us to *individualize* Crocus, and to distinguish it

from all the other drugs which in many respects agree with it.

It will be observed that dark-colored uterine hæmorrhage, though produced by Crocus, cannot be said to be *characteristic* of it. It is a characteristic symptom of a group to which Crocus belongs, but not of Crocus, for it is produced by the other members of this group as well as by Crocus.

Characteristic symptoms must of necessity be for the most part subjective and seemingly trivial phenomena. A list of them alone, if presented as the pathogenesis of a drug, would be as meaningless, and at first sight as ridiculous, as a list of the colors and marks and angles and curves by which friends recognize each other would be, if presented alone as the sum total of the properties of certain genera and species of the animate creation. As a background to the latter, there must be a series of phenomena capable of morphological and organic arrangement, and as the basis of the former we must have a series of objective and organic symptoms capable of physiological and pathological arrangement and of approximate explanation. But it must never be forgotten that *without* the *characteristics*, as we have described them, there can be no *individualization*, and without *this* there can be no *accurate homœopathic prescription.*

The truth of this is made apparent by a glance at the history of Homœopathy. Certain of Hahnemann's followers discarded the apparently trivial subjective phenomena from the provings of drugs, retaining only the objective, organic symptoms. They thus lost the means of distinguishing between the individual members of the groups of remedies. It was thenceforward useless for them to discriminate closely between individual cases of any one type of disease. Hence, inevitably, arose the fashion of prescribing a *specific* remedy for a disease,—as the phrase went,—putting the leading members of respective groups of drugs and diseases to represent the whole groups. These were the so-called "specifikers,"—who had one or two remedies for dysentery, one for

hooping-cough, one or two for scarlatina, etc., "of whom the world is weary."

But the characteristic is not always a *definite symptom.* Sometimes it is so,—as in the case of Crocus, and as in the peculiar diplopia of Stramonium. But sometimes it resides in a peculiar condition which attaches to some symptom common to two or more drugs. This condition may be of time, or circumstance, or concomitance. Thus, if two drugs have the symptom "dry cough from tickling in the supra-sternal fossa,"—but to one is added the condition "occurring only in the evening,"—this condition of time is the characteristic of that drug in so far as the dry cough is concerned; or if one have this condition of circumstance, that "the cough is aggravated by inspiring cold air,"—*this condition* is the characteristic; or if one have the *concomitant* that the cough is attended by retching,—this condition of concomitance is the characteristic.

Sometimes the characteristics resides in the *conditions collectively.* We borrow examples from Dr. Drysdale's admirable Introduction to *The British Repertory:*

"Pain in the stomach with nausea occurs under twenty-eight medicines.

"Pain in the stomach in the morning under thirty-seven.

"Pain in the stomach with nausea in the morning under four only."

Or it may reside in a concomitant.

"Dry retching occurs under forty-five drugs.

"Dry retching in the morning under five.

"Dry retching with eructation under one only—Ledum."

Every drug-proving, then, is to be studied in a twofold way: On the one hand, so as to enable us to attach it in our memory to certain groups of drugs to which it shows marked general resemblances; and, on the other hand, so as to bring out clearly into view those characteristics which distinguish it from all the other drugs of these groups in

particular and of the Materia Medica in general. Our study will be at once synthetic and analytic.

Such a study is of necessity *comparative* in its nature. Each positive step in the study of a drug involves a question of the correspondence or difference of other drugs in respect of that step. An isolated study of all the remedies would not give us an available knowledge of the Materia Medica. It is not enough to know that Pulsatilla, Nux vomica and Chamomilla each produce diarrhœa of a certain kind. We must also know and fix in our minds the similarities and differences of each of these diarrhœas to those of the two other and of all other drugs. The study of one drug is, in fact, then, the study of the whole Materia Medica. One is never so competent to thoroughly master a proving as when he has already mastered all other provings. The first effort must necessarily be the least satisfactory, the most imperfect.

This is the task to which the student of Materia Medica is invited and at which his predecessors have been laboring for fifty years. Why, he may ask, has not this been wrought out and systematized by those who have gone before? Why is the Materia Medica left in the same state in which Hahnemann placed it fifty years ago?

Our Materia Medica consists of the provings of drugs upon the healthy, made by Hahnemann and his disciples. These provings, as we have them, are, for the most part, a formal arrangement of the symptoms subjective and objective observed by the prover or his friends. No attempt is made, with but few exceptions, to trace any pathological connection between symptoms, or to give any physiological explanations, or to distinguish between characteristic and generic symptoms. The symptoms alone are given, just as the symptoms of a case of disease would be given by an intelligent but uninstructed patient who unfolds his case to us in as plain untechnical words as he can, leaving to us the task of tracing connections and contriving explanations. There

they stand, records of facts made in the plain vernacular, intelligible so long as the language shall endure.

But Hahnemann had a much higher idea of the kind of knowledge of Materia Medica which a physician requires than this statement would imply. In an essay on "The Power of Small Doses," in Hufeland's *Journal*, he describes this knowledge as follows: "What organs it (the drug) deranges functionally, what it modifies in other ways, what nerves it principally benumbs or excites, what alterations it effects in the circulation and digestive operations, how it affects the mind, how the disposition, what influence it exerts over some secretions, what modification the muscular fibre receives from it, how long its action lasts, and by what means it is rendered powerless, etc., etc." Why, then, did he not construct his Materia Medica on this model? Unquestionably because, with a wonderful sagacity, which together with his brilliant genius and his prodigious learning made him the "double-headed prodigy," which Jean Paul Richter called him, Hahnemann clearly perceived the following truths: that the *positive facts* with which a physician has to deal in *constructing* a Materia Medica are the *observations* of the *prover*, recorded in plain, unfigurative, non-hypothetical language. That the construction which he saw to be so desirable must be the result of the *application* of the sciences of *Physiology* and *Pathology* to these *facts*. That the facts of the proving being of the nature of *positive observation* are *enduring* and *unchangeable*. But that the *sciences* of *Physiology* and *Pathology*, being *incomplete* and *progressive* are *continually undergoing change*, and that their *terms* must therefore be ever *varying* in significance as the theories on which the sciences are based vary. That, consequently, a Materia Medica constructed by him out of these two elements, one constant and the other variable, would of necessity be transient,—could not be enduring,—would soon grow obsolete and in its decline would carry out of sight the constant element also, and thus the labor of the

provers would soon be lost to the world. Such a structure would have involved an *intermingling* of the *current physiological theories* with the *facts* derived from *observation*. The precise point and extent of the intermingling would soon become undistinguishable, and thus a vitiated record would be transmitted to posterity such as the advance of science would soon render useless. A comparison of the present state of Physiology with that of 1800, of which the very terms are almost obsolete, makes the great wisdom of this view apparent. On the other hand, the pure records of observed facts, untainted by theoretical speculations, come to us from the Master's hand as pure, as intelligible, as available as when first recorded.

We have the same material for the construction of a physiological theory of the drug-action that Hahnemann had, and we can construct it with the advantage in our favor of the great advances which Physiology and Pathology have made since Hahnemann's day. This is the work which each of us must do for himself. No other can do it for him. The result of his labor may and will differ somewhat from that of every other student, for with the light of the auxiliary sciences he forms a judgment concerning observed facts, *and the significance of a fact is measured by the capacity of the observer*.[1]

The student should seek his knowledge of Materia Medica at the fountain head, in the original publications of Hahnemann's *Materia Medica Pura* and *Chronic Diseases*, in the provings in Stapf's *Archiv*, and in the Austrian and other journals.

The *Manuals*, however convenient for reference in the hurry of practice, are not suitable for systematic study. In some of them, the phraseology of the prover has been altered.

[1] Lest by an omission I expose myself to misconstruction, I may say that inasmuch as advances in collateral medical sciences are affording continually new aids to observation, it is incumbent on each generation to re prove to a certain extent the remedies of the Materia Medica so as to bring these aids to bear on the study of Pathogenesy.

In others, the symptoms as reported by the prover, have been arbitrarily sundered into fragments, and these fragments are scattered throughout the record. Or symptoms *ex usu in morbis* have been introduced, and the names of diseases supposed to have been cured by the drug are incorporated with the pure symptoms. In all of them the arrangement is somewhat altered. In many, attempts at abbreviation have been made, and with no better success than if one should squeeze one's lemons to lessen the bulk of one's luggage and yet hope to have good lemonade at the end of one's journey; for it always happens, and must from the nature of the case, that the *skins* are the part *retained* while the *juice* is thrown away.

If a Manual *must* be employed, that of Noack and Trinks seems preferable; for it preserves the phraseology of the prover and does not to any great extent sunder groups of symptoms, while it places under distinct headings the pure symptoms, and the clinical effects of the drugs and the theoretical speculations of the compilers, so that the student is in no danger of mistaking the one for the other, a danger to which Jahr's *Manual* does certainly expose him, and for which reason Jahr's work is less desirable than that of Noack and Trinks.

We have dwelt at some length on the sources from which the student should seek his knowledge of Materia Medica—and with good reason! "For, can a bitter fountain send forth sweet waters?" "Do grapes grow on thorns, or figs on thistles?" If the student should fall among false or incompetent teachers, could the doctrine and practice he learns be true and successful? Now, it will be observed that the records and provings and the manuals of Materia Medica to which we have commended the student are all German works, while nine-tenths of our American and English practitioners and students are unacquainted with the German language.[1] It is humiliating to us to be compelled to say

[1] The Author considered a knowledge of German almost indispensable to a student of Homœopathy.—Ed.

that there are no trustworthy manuals in the English language, and no translations of the German works which we have named on which reliance can be placed. We have translations of Hahnemann's *Materia Medica* and *Chronic Diseases* and of Stapf's *Contributions to the Materia Medica*, by Dr. Hempel. But, either (perhaps we should say *both*) from lack of moral capacity, or of intellectual and professional acquirements, or from haste and carelessness, the translator has so marred these works by errors of omission and of translation that they are to the student and practitioner what false lights on a difficult coast are to the unsuspecting mariner.

Dr. Hempel translated also Jahr's *New Manual* or *Symptomen Codex*, and claims to have incorporated with it nearly all that is valuable in the Manual of Noack and Trinks, thus making it the most complete and perfect work of the kind in existence, and the best possible in the present state of our science. This translation was published with a preface by Dr. C. Hering, and under the high sanction of his name and that of Dr. Gray, of New York. It is almost universally used in this country and in England, and the knowledge of the Materia Medica and of the true practice of Homœopathy possessed by the rising generation of practitioners is, in the main, what this manual is capable of affording.

If now, I should say that this translation and compilation contains all the errors, omissions and perversions which rendered Dr. Hempel's former translations untrustworthy, along with many others peculiar to this work, I should surely hear in reply that Dr. Hering has expressly in his preface commended the diligence and accuracy and zeal of the translator, and I should be referred to page six of the preface,[1] where Dr. Hering is made to say, "The Editor has, with *extraordinary minuteness and labor*, compared the two manuals, and has transferred to the manual of Jahr all additional drugs and pathogenetic effects contained in Noack and Trinks' work." What could I say in offset to this indorse-

[1] Jahr's *New Manual* or *Symptomen Codex*, pref. vi.

ment of Dr. Hering? There is a word to say on the subject of this preface. It was unquestionably written originally by Dr. Hering in German. The English translation, by whomsoever made, seems not to have been altogether satisfactory to Dr. Hering (as well it might not be), for the German original was published by him (a "*verbatim* copy," as he says) in the *Allg. Hom. Zeitung*. The English preface to Jahr's *New Manual* differs from this German original far more widely than the license of scholarship will at all justify. In a foot-note to the original of the passage we have quoted above in commendation of the Editor and translator of Jahr's *New Manual*, Dr. Hering says:[1] "After comparison of the translation with the original, the above indorsement is hereby altogether and completely withdrawn."[2] Thus, in an obscure foot-note in a German periodical, seen by not more than one in a hundred of those American and English students who are induced by Dr. Hering's commendatory preface to place confidence in Dr. Hempel's translations and compilations, Dr. Hering vindicates his fame as a good scholar and a faithful champion of our science by withdrawing his commendation of Dr. Hempel.

If those whose confidence has been misplaced by reason of this indorsement in English, cancelled in German only, should, with a measure of just indignation, demand of me why the retraction was not made in the language and in the country in which the commendation was suffered to be published,—why Drs. Hering and Gray, acknowledged leaders of our school in America, did not suppress this so-called translation or expose its false pretensions, in words as intelligible and as widely read as those in which their support of it was permitted,—alas! I have nothing to say. (See Appendix.)

To plead that, in a matter which involves the education or perversion of a whole generation and the healthy progress

[1] Vol. XI., Nos. 23 and 24, February and March, 1851.

[2] Dies wird hiermit nach Vergleichung der Uebersetzung mit dem Original ganz und gar zurückgenommen.

and soundness of Homœopathy wherever the English language is spoken,—in such a matter the engrossments of business or tenderness toward a delinquent individual kept them silent, would be to cast a doubt upon the estimate in which they hold scientific truth, a doubt inadmissible in gentlemen of their position.

It avails not to say: Why find fault with these translations and this manual, inasmuch as we have no others? Had the unworthiness of these been made known, had they not, on the contrary, been indorsed by high authority, we had long since had others and trustworthy. An exposure of the imperfections we have spoken of would have created a demand for other works, and it is not less true in science than in trade that "demand creates a supply."

Having selected a remedy on which to commence his studies, the student should gather together all the reports of provers, whether in the form of their daily records (in which form our dear and lamented colleague Dr. Joslin published his admirable proving of Rumex crispus, as did also the Austrian provers), or in the Hahnemannian anatomical scheme, and should carefully peruse them. We will assume that he has selected Pulsatilla, and will use this remedy to illustrate what we have further to say. We have no other proving of this drug than the very perfect one of Hahnemann in vol. i, of *Materia Medica Pura.*

During the first perusal,—and several may be necessary for the purpose,—the student should endeavor to make a *general* analysis of the proving. This analysis would enable him to place the drug along with several others in one or other of certain groups, into which he will find, as he advances in his studies, the Materia Medica arranges itself. Among the chief points of this general analysis will be the following:

I. *Sphere of action in the drug.* It will be seen that every drug affects some organs or systems of organs or tissues

more decidedly than others. Pulsatilla, for example, acts pre-eminently upon the vegetative system, upon the organs of reproduction and their appendages, and upon the composition of the blood, depressing the action of the former systems and producing in the latter a condition similar to that of one form of chlorosis. We learn these facts by bringing a knowledge of Physiology to bear upon and interpret the symptoms of the intestinal tract and of the urino-genital organs, those of the vascular system and the symptoms of the head and disposition. For in these we have retarded digestion, vertigo, audible pulsation of the carotids, momentary loss of sight and hearing on sudden exertion, palpitation, paleness, retarded and scanty menstruation with syncope and exhaustion; depressed melancholic disposition. On the other hand, the student will notice that Pulsatilla exerts but little action upon the bones, skin and glands, and this will be another important step toward grouping.

II. *The extent to which the organic substance is affected.* From some provings it must be gathered—Spigelia for example—that the organic substance is but slightly affected or only in isolated localities, while in other provings the effect is profound and general,—Carbo vegetabilis and Lachesis. In others, again, the affection of the organic substance and the irritation of the nervous system are equal in degree and both are great,—Arsenicum. Conclusions on this head are drawn from the following symptoms: those of the complexion and of the skin generally, as regards color and temperature, which enlighten us respecting congestions, if there be any, and the color and character of the congesting fluid; those of the evacuations from the bowels, bladder, uterus, and all secreting glands and surfaces; those of the cutaneous eruptions and ulcers; finally, those which denote the existence of dyscrasias of whatever variety, *e. g.*, dropsies, phthisis, cancer, gout, rheumatism, etc. Under this head we find in the proving of Pulsatilla no evidence of any

further action than that above mentioned—a hydræmic dyscrasia, and which is further corroborated by the abundant serous or thin mucous discharges from secreting glands and surfaces.

III. *The action of the drug on the vital power*, correlative of the above, and shown in the symptoms of the nervous system as they are given conjointly with the symptoms of the various organs to which the different parts of the nervous system are distributed. He may consider the nervous system under five heads. *a.* The *sensorium*, of which the symptoms are found chiefly under the rubrics Head and Disposition. *b.* The *general sensibility*. *c.* The *general mobility*. Data respecting these heads are found in the symptoms of the tissues to which the nerves of sense and motion are distributed. *d.* The *special sensibility*, as exhibited in the symptoms of the organs of *special sense*—the eye, ear, nose and tongue. *e.* The *sympathetic system*—as exhibited in the symptoms of organs containing involuntary muscles—in the intestinal tract and in all the sphincters.

In forming conclusions on any one of these points, regard must be had to the entire remaining action of the drug. We should otherwise reach a very false judgment. Pulsatilla, for example, produces blindness and deafness. We might regard these as very important affections of the special senses, did we not learn also that these phenomena occur simultaneously with scanty and difficult menstruation, and with palpitation and throbbing of the carotids, and conjointly with great pallor and frequent syncope. These occurrences compel us to regard the blindness and deafness as sympathetic symptoms occurring in a chlorotic patient, and connected perhaps with a hydræmia produced by Pulsatilla.

These three sections of a general analysis having been elaborated during a first perusal, the student will already be in a position to arrange many drugs in groups by their similarities and differences in these respects. He will note, for example, that in their *sphere* of action Pulsatilla and Nux

vomica are closely allied, while again they differ widely in their *mode* of action both on the organic substance and on the vital power, etc.

The practical use of such an analysis is this: that when such fundamental facts are known of two or more drugs, it is enough to have clearly in mind, in any case of disease, what are the effects of the disease in these three fundamental respects. If, then, the action, for example, on the organic substance be similar to what we have seen to be the effect of Pulsatilla, there can be no possibility of Nux vomica being applicable in the case, and no need, therefore, of studying that drug further for the case. This process of elimination by means of a general analysis may be relied on wherever we have good and complete provings of drugs, and where the case of disease presents clear and definite symptoms. When we are dealing with fragmentary provings and obscure cases it is of course not practical.

As further examples, we may adduce the following: Spigelia and Silicea both affect the special senses remarkably and similarly; yet they are extremely different in their action on the organic substance. Hyoscyamus and Carbo vegetabilis affect the sphincters similarly, though in other respects so unlike.

After thus generalizing in a comprehensive way, the student will observe certain phenomena of a more special character; for example, that among the variety of sensations recorded as having been produced by the drug, there is a certain uniformity in general character throughout most of the organs affected. But here he meets difficulties arising from inaccuracy of provers, or rather from the fact that, all descriptions of *sensations* being clothed in figurative language, the imaginations of different provers suggest to them different modes of expression. The wealth of the German language in synonyms has not diminished this difficulty. An approximate analysis of sensations such as has been made by Dr. Dudgeon would be of service in this regard. The

individuality of some drugs is much more strongly marked than that of others by this feature of their effects, and *pro tanto* it serves as their characteristic. Thus Bryonia and Squilla are distinguished by *sticking*, and Arsenicum by *burning*, *pains*.

Another point to be noted, and which may serve still further to individualize the drug, is periodicity, which in many drugs is well marked and of a definite type; *e. g.*, Arsenicum, Ipecacuanha, Natrum muriaticum, Nux vomica. In Pulsatilla it is very marked, but the type is not constant.

But, perhaps, the most important of all the considerations in which resides the individuality of a drug are the conditions and concomitants of the symptoms. The conditions are the phenomena of time, place, and circumstance on which the symptoms depend. For example, Pulsatilla produces tearing pain in the hip. So do several drugs, but that of Pulsatilla occurs in the *afternoon*,—condition of time; it occurs and is aggravated in a warm room,—condition of place; occurs during and is aggravated by repose, —condition of circumstance. A concurrence of this phenomenon and these conditions is found only under Pulsatilla. Here these conditions are the *characteristic*. The concomitants are those phenomena, whether we call them sympathetic or secondary, which always accompany any symptom or group of symptoms. Absence of thirst is a concomitant of many groups of symptoms under Pulsatilla. So likewise are chilliness, cold feet, wakefulness in the evening and sleepiness in the morning, etc. Nux vomica has the reverse. So is cold sweat of the forehead under Veratrum.

Having thus made a general analysis of the proving, obtaining, first, a general view of the *action* of the drug on the great divisions of the organism and of the pathological conditions which it produces; and, second, a general view of the *characteristics of its action*, the student may proceed to a special analysis, which will involve a similar study of the action of the drug on each organ and anatomical region of

the body. In this he cannot do better than follow the Hahnemannian scheme.

The points to be considered in each region and under each rubric are the following: The organic changes; the sensations,—their nature, locality and direction; the conditions of time, place and circumstance, and the concomitants. Thus, for example, in studying the Head, he may consider:

a. The SENSORIUM, under which the subdivisions may be:

1. Vertigo, its nature, conditions of time, place and circumstance, and its concomitants. Thus, the vertigo of Pulsatilla is a staggering; it occurs after eating, in a warm room, and during repose (there is a rare alternate effect); the concomitants are heaviness in head on stooping, paleness and internal heat in the head.

2. Intelligence, with conditions and concomitants as above.

3. Memory, " " "

4. Illusions of the imagination, " "

b. HEADACHE, under which the points to be noted are:

1. Character of the pain.

2. Locality.

3. Its course if it move.

4. Conditions and concomitants as above.

c. THE ORGANIC CHANGES, which are to be studied in the same way. These comprise all objective and material phenomena.

In this manner, the student will examine the effect of the drug upon each organ and tissue of the body, as will be more clearly shown by a scheme for the study of the Materia Medica which will be appended to this essay.

The result will be an accurate knowledge of the action of

the drug is so far as the proving is complete, upon the whole organism in general and in detail. The special analysis will serve to correct certain errors into which the general analysis might lead the practitioner. Conditions are not always uniform for all the organs. Thus, although the *general* conditions of Pulsatilla are occurrence and aggravation in the afternoon and evening, during repose and by heat, with relief by motion and cold, and must be so stated in a general analysis, yet there are a few *special* symptoms to which the opposite conditions attach, and this fact is brought out by the special analysis.

The study of a proving to be practically available must be comparative. After ascertaining the properties of each drug by positive investigation and analysis in the manner detailed, the next step is to ascertain what drugs resemble it, and in what features they are like and how they differ. To make such a comparison as this in studying the Materia Medica, a repertory is indispensable, and this need alone, if a repertory were not equally indispensable in daily practice, would be a sufficient reply to those who idly talk about such a work being superfluous or mischievous.

Such is a method of studying the Materia Medica which, after much reflection and trial of various plans, I venture with unfeigned diffidence to unfold. It is elaborate and requires a wearying application which those alone can appreciate who have engaged in similar tasks. To complete such a systematic study, even in comparative leisure, might require seven years of unremitting labor—just the period for which a lad is apprenticed to learn his trade. Should we shrink from devoting so long a time to the mastery of the most complex and difficult, and the noblest science and art which are possible to man on earth?

I desire to add a few words of a practical nature. Prescribers are liable to two errors of an opposite kind; the possibility of which will be apparent from what has been written.

The one consists in prescribing from a general analysis of drugs without regard to the characteristics which individualize them. This is equivalent to prescribing any member indifferently of a whole group of drugs, and necessitates a corresponding generalizing view of disease. It is the method of the Old School which seeks to arrange drugs and diseases in groups and which ignores characteristics and individuals.

The other error consists in prescribing on the strength of one or two characteristics which may be detected, without however examining whether the general effects of the drug correspond to the general features of the disease. Now, characteristics, for the most part, as we have seen, derive their value from their association as concomitants or conditions with some symptom which is not in itself a characteristic. Disconnected from this, they are as void of significance as a man's nose would be if cut off from his face, though while on his face it might have been the chief feature by which his friends recognize him. I will not deny that by this method, great successes, "lucky hits" I would call them, are sometimes made. But I do stoutly affirm that atrocious and inexcusable blunders are much more frequently the result.

I admit, too, that in certain cases of disease there is no possibility of making such an analysis as we have advised, and that certain drugs are so incompletely proved that we know of them only one or two characteristic symptoms, and cannot study them as recommended. All that can be said of such cases is that they are incomplete and come under no rule. We must do the best we can and adopt a defective method, which is nevertheless sometimes successful rather than make no attempt to cure. Better cure by a "lucky hit" than not at all. But let not this lead us astray where we might do better.

If one had to traverse a wilderness he would desire first of all a compass. If this were not to be had he might "steer by the stars." If these were obscured he might judge from the direction of vegetation and of hills and rivers. Failing

these, he might even "guess" and his guess might lead him right. Nevertheless, few travelers of sound mind would be led by such a success to prefer a "guess" to a "compass."

A SCHEME FOR THE STUDY OF MATERIA MEDICA.—In the hope that what has been written may serve not only to explain the method in which I have thought the study of Materia Medica may be best pursued, but also to assist those who are entering upon that arduous study, I publish herewith a schedule drawn up originally for my own guidance, and which, I think, was a great assistance to me.

A.—GENERAL ANALYSIS.

1. ACTION ON THE VITAL POWER—as exhibited in the action of the remedy upon—*a*. The sensorium; *b*. The special senses; *c*. The sphincters; *d*. The other involuntary muscles; *e*. The power of locomotion.

2. ACTION ON THE ORGANIC SUBSTANCE; as exhibited in *a*. The complexion (showing, viz.: the state of vessels and the nature of their contents); *b*. The evacuations; *c*. Ulcers, if any which previously existed are modified by the remedy or if any are produced by the remedy; *d*. Eruptions; *e*. General affections of a dyscrasic nature, such as dropsy, tuberculosis, etc., etc.

3. THE SPHERE OF ACTION OF THE REMEDY. What organs or systems of the body are affected in a general way and in what order they are affected.

4. SENSATIONS. What kind of sensations predominate among those ascribed to the remedy in the proving, and what relation, if any, appears to exist between certain varieties of sensation and certain classes of organs or tissues.

5. PERIODICITY. If there be any periodicity in the symptoms it is to be particularly noted and its character

6. PECULIARITIES. There are few good provings which do not ascribe to the remedy certain peculiarities of action which are incapable of classification, which run through the whole action of the remedy and which are peculiar to one remedy and are therefore important characteristics.

These *peculiarities* constitute the *conditions of time, circumstance, aggravation* and *concomitance,* which are attached to the symptoms and often give individual character to them as belonging to one remedy in particular, and as incapable of being ascribed in mass to any other drug. A symptom being recorded as produced, for example, by Pulsatilla, the questions "When did it occur?" "When was it aggravated?" "When was it ameliorated?" "When did it cease?" give the condition of time which attach to that symptom. In like manner the questions "Under what circumstances (viz., of rest or motion, of heat or cold, etc., etc.) did it occur, was it aggravated, was it ameliorated," give the conditions of *circumstance* and *aggravation* which attach to the symptom.

It will be observed of almost every remedy that certain symptoms or series of symptoms are accompanied by one or several other symptoms. This fact is the condition of *concomitance* to which we refer. For example, many symptoms of Pulsatilla are accompanied by a disposition to weep, of Nux vomica by *fugitive chills,* of Arsenicum by inordinate weakness, of Veratrum by cold sweat of the forehead, etc.

It is further noticeable that in the provings of some drugs this concomitance of symptoms by other symptoms is strongly marked, and almost universal as in the case of Arsenicum.

7. RESUMÉ OF THE CHARACTERISTICS OF THE REMEDY. This will include everything in the above analysis which is shown by a comparative study of other drugs to be peculiar to it. It will comprise, in particular the conditions of time, circumstance, aggravation and concomitance, and the *periods* of the day at which the action of the drugs is most marked.

B.—SPECIAL ANALYSIS.

1. HEAD.

a. *Sensorium*, comprising the subdivisions: 1. Vertigo; 2. Intelligence; 3. Memory; 4. Illusions. Each of these subdivisions is to be studied in respect of *sensations* in so far as they can be predicated of it; of *periodicity*; of *conditions of time, circumstance, aggravation* and *amelioration*; and of *concomitance*. And under the latter rubric it is to be especially inquired what symptoms affecting other organs, or systems, occur simultaneously in evident physiological connection with the symptom under consideration.

b. *Headache.* The points to be investigated are: 1. The *location* of the pain if it be stationary—its *course* if it move; 2. The sensation; 3. The conditions of time, circumstance, aggravation and amelioration, as above explained in detail; 4. Concomitance.

c. *Organic changes.* These affect the material substances and tissues of the head, and comprise all deviations from a normal material condition of which we can in any way become cognizant—affections, for example, of the skin, glands, vessels, organs of special sense, etc.

2. EYES.

a. *The Orbit.* 1. Location; 2. Sensations; 3. Conditions; 4 Concomitance; 5. Organic changes. These subdivisions here and throughout the scheme, wherever used, are to be understood and studied as already explained in the general analysis and under the title "Head."

b. *Lids.* 1. Location; 2. Sensation; 3. Conditions; 4. Concomitance; 5. Organic changes.

c. *Conjunctiva.* 1. Location; 2. Sensations; 3. Conditions; 4. Concomitance; 5. Organic changes.

d. *Globe.* 1. Location; 2. Sensations; 3. Conditions; 4. Concomitance; 5. Organic changes.

e. *Secretions.* 1. Character, as regards color, acridity, quantity, etc.; 2. Conditions; 3. Concomitance.

f. *Special sense.* 1. Vision, altered, intensified, diminished, or perverted. 2. *Conditions* attached to such alteration; 3. Concomitance.

N. B. After each such study of a single organ, a physiological estimate should be made of the significance of the symptoms. For this purpose a faithful study of the concomitance is of inestimable value. For example, the proving of Pulsatilla gives sudden loss of vision. Is this Amaurosis? The concomitance shows that the loss of vision is accompanied by loss of hearing, by syncope, and occurs at a period of menstrual irregularities. The symptom is at once explained. This is only a very obvious instance of what is not always an easy though always a most necessary task.

3. EARS.

a. *External Ear.* 1. Location; 2. Sensations, 3. Conditions; 4. Concomitance; 5. Organic changes.

b. *Internal Ear.* 1. Location; 2. Sensations; 3. Conditions; 4. Concomitance; 5. Organic changes (as secretions, etc.) and their conditions.

c. *Special sense.* 1. Deviations, whether intensified, diminished or perverted; 2. Conditions; 3. Concomitance.

4. NOSE. 1. Location; 2. Sensations; 3. Organic changes, including secretions. With reference to each of the above, observe, a. The conditions; b. The concomitance.

Special sense of smell. 1. Deviations from a normal condition; 2. Conditions; 3. Concomitance.

5. FACE. 1. Location; 2. Complexion; 3. Sensations; 4. Temperature; 5. Organic changes. As regards each of these subdivisions, study, a. The conditions; b. The concomitance.

6. LIPS. 1. Location; 2. Aspect; 3. Sensations; 4. Temperature; 5. Organic changes. Respecting each subdivision, note, a. The conditions; b. The concomitance.

7. GUMS AND TEETH. 1. Location; 2, Sensations; 3. Organic changes. In reference to each subdivision, note, a. The conditions; b. The concomitance.

8. MOUTH. 1. Location; 2. Sensations; 3. Organic changes. Of these, a. The conditions; b. Concomitance.

9. TASTE. 1. Deviation from normal state. a. Conditions; b. Concomitance.

10. TONGUE. 1. Location; 2. Sensations; 3. Aspect; 4. Organic changes. Of these, a. The conditions; b. The concomitance.

11. THROAT AND FAUCES. 1. Location; 2 Sensations; 3. Aspect; 4. Organic changes (including tumefaction, secretion, deposits, etc.). Respecting these divisions, a. The conditions; b. The concomitance.

12. ŒSOPHAGUS. 1. Location; 2. Sensations; 3. Organic changes; 4. Special function. Of these, a. The conditions; b. The concomitance.

13. STOMACH. 1. Location; 2. Sensations; 3. Organic changes; 4. Special function modified, as respects, a. *Appetite;* b. *Thirst;* and these modifications may be exaltation, diminution or perversion; c. *Nausea;* d. *Vomiting.* Respecting these divisions and subdivisions, observe, a. The conditions, b. The concomitance.

14. HYPOCHONDRIA. 1. Right, 2. Left. As regards each, study; 1. Location; 2. Sensations; 3. Organic changes. As regards each subdivision, observe, a. The conditions; b. The concomitance.

(Here and wherever the rubric comprises a region which contains a number of organs, endeavor to make by the aid of the sciences of Physiology and Pathology a rational

appreciation of the organs involved, and in this study, pay especial regard to the concomitance, in order to gain the full physiological significance of the symptoms. For example, in affections of the right hypochondrium where the liver may be supposed to be implicated, the concomitance will justify the supposition if it show simultaneous and corresponding symptoms of the tongue, stomach, head, stool, skin, back, etc., etc. So of the left hypochondrium and the uterine system.)

15. ABDOMEN. 1. Location with appreciation of the organs involved; 2. Sensations; 3. Organic changes. Respecting these divisions, note, a. The conditions; b. The concomitance.

16. STOOL. 1. Sensations, their character, location and course. a *Before* stool; b. *During* stool; c. *After* stool. As regards these three subdivisions, note, a. The conditions; b. The concomitance. 2. *Organic phenomena*, viz, color, odor, consistence, composition, *e g.*—digested or not—consisting of fæces alone or combined, etc.

17. ANUS AND RECTUM. 1. Location; 2. Sensations; 3. Organic changes (including secretions, tumors, condylomata, etc., etc.). Respecting all of these divisions, note, a. The conditions; b. The concomitance.

18. URINARY ORGANS.

a. *Bladder.* 1. Location 2. Sensations; 3. Organic changes.

b. *Urethra.* 1. Location; 2. Sensations; 3. Organic changes.

c. *Kidneys.* 1. Location; 2. Sensations; 3. Organic changes.

d. *Ureters.* 1. Location; 2. Sensations; 3. Organic changes.

With regard to all of these divisions, under each of these organs, study, a. The conditions; b. The concomitance.

e. *Urine.* 1. Quantity; 2. Color—physical propertie: 3. Odor; 4. Deposits.

f. *Micturition.* 1. Frequency; 2. Sensations. Note, a. Conditions; b. Concomitance.

19. GENITAL ORGANS—*Male.*

a. *Penis.* 1. Location; 2. Sensations; 3. Organic changes.

b. *Testes.* 1. Location; 2. Sensations; 3. Organic changes.

c. *Special function.* 1. Modifications, as regards exaltations, diminution, or perversion; 2. Sensations

Respecting these divisions and subdivisions, study under each, a. The conditions; b. The concomitance.

d. *Secretion* 1. Quantity; 2. Quality, admixture, etc.; 3. Mode of evacuations. a. Conditions; b. Concomitance.

20. GENITAL ORGANS—*Female.*

a. *Vulva.* 1. Location; 2 Sensations; 3. Organic changes.

b. *Vagina* 1. Location; 2. Sensations; 3. Organic changes, 4 Secretion, considered as regards, a. Character; b. Quantity; c. Color.

c *Uterus.* 1. Location; 2. Sensations; 3. Organic changes, 4. Secretions considered as above.

d. *Ovaries.* 1. Sensations; 2. Organic changes. Under these divisions and subdivisions, note, a. The conditions; b. The concomitance, attaching to each.

e. *Menstruation.* 1. Sensation; 2. Periodicity; 3. Quantity; 4 Color; 5. Consistence; 6. Duration. Under these, a. The conditions; b. The concomitance.

21. RESPIRATORY ORGANS.

a. *Nasal Mucous Membrane.* 1 Location; 2. Sensations; 3. Organic changes; 4. Secretions, as regards quantity and quality. Respecting all these, a. The conditions; b. The concomitance.

b. *Larynx and Trachea.* 1. Location; 2. Sensations; 3. Organic changes; 4. Secretions as above. Respecting all these, a. The conditions; b. The concomitance.

1. Cough. a. Its sound; b. Its character, as paroxysmal or otherwise, dry or loose; c. Location of the sensation which provokes it; d. Conditions; e. Concomitance.

2. Sputa. 1. Character; 2. Consistence; 3. Color; 4. Smell; 5. Taste; 6. Locality from which it appears to come; 7. Mode of evacuation; 8. Sensations which precede and follow its evacuation; 9. Conditions; 10. Concomitance.

c. *Thorax—Internal.* 1. Location; 2. Sensation; 3. Organic changes. a. Conditions; b. Concomitance.

—*External.* 1. Location; 2. Sensation; 3. Organic changes (including mammæ and nipples); 4. Secretions. Respecting the above, a. Conditions; b. Concomitance.

d. *Respiration.* 1. Sensations; 2. Organic action; 3. Breath, its odor, temperature, volume. Regarding these, a. The conditions; b. Concomitance.

22. HEART

a. *Subjective.* 1. Location; 2. Sensations.

b. *Objective.* 1. Location; 2. Organic changes as shown by position, sounds, magnitude and by rational signs; 3. Pulsation. Respecting these divisions, a. The conditions; b. The concomitance.

23. BACK. 1. Location, dividing the back into regions from below upward and appreciating the organs and tissues involved, as well as the concomitance will admit; 2. Sensations; 3. Organic changes. Under these, a. Conditions; b. Concomitance.

24. UPPER EXTREMITIES—RIGHT AND LEFT. Respecting each, study

1. Articulations. 1. Location; 2. Sensations; 3. Organic changes.

2. Interarticular Region. 1. Locations; 2. Sensations; 3. Organic changes. Respecting all of these, a. Conditions; b. Concomitance.

25. LOWER EXTREMITIES—RIGHT AND LEFT. Studied in all respects as the Upper Extremities.

As regards the Extremities generally, note Sensations with their course (and Duration) if they be wandering; *e. g.*, from a right upper, to a left lower extremity, etc.

26. SLEEP. 1. Character; 2. Periods; 3. Preceded by; 4. Succeeded by; 6. Concomitance.

a. *Dreams.* 1. Conditions; 2. Concomitance.

27. FEVER, as a paroxysm made up of stages.

1. *Incomplete Paroxysm,* consisting of only one or of two stages. 1. Their order; 2. Relative severity and duration; 3. Antecedents; 4. Sequelæ; 5. Conditions; and, 6. Concomitance.

2. *Complete Paroxysm.* 1. Order of the stages; 2. Their relation to each other. Then consider the separate stages; *e.g.*:

a. *Chill.* 1. Character; 2. Precursor; 3. Succeeded by; a. Conditions; b. Concomitance.

b. *Fever,* as a single stage, or *Heat.* 1. Character; 2. Precursors; 3. Sequelæ. a. Condition; b. Concomitance.

c. *Sweat.* 1. Character; 2. Precursors; 3. Sequelæ. a. Conditions; b. Concomitance.

Between chill and heat and heat and sweat, study the

Interval. 1. Character; 2. Sensations; 3. Conditions; 4. Concomitance. Also the time

After the paroxysm. 1. Sensations; 2. Conditions; 3. Concomitance.

Pulse. 1. Quality; 2. Frequency; 3. Conditions.

28. SKIN.

1. General. 1. Sensations; 2. Organic changes; 3. Conditions; 4. Concomitance.

2. *Special.* 1. Localities affected; 2. Sensations; 3 Organic changes; 4. Conditions; 5. Concomitance.

3. *Organic Changes.* Under this head are comprised:

1. *Eruptions.* 1. Aspect; 2. Color; 3. Sensations; 4 *Secretions.* a. Their color; b. Consistence; c. Odor; d Quality—*e.g.*, acrid or bland, etc.; 5. *Scabs.* a. Quality b. Tenacity; c. Color; d. Odor; e. Conditions of subjacent tissues or secretions.

2. *Ulcers.* To be studied as Eruptions.

3. *Warts.* " " "

4. *Tumors.* 1. Physical properties; 2. Sensations; 3 Conditions; 4. Concomitance.

29. MIND.

1. *Faculties Modified.* 1. Exalted; 2. Depressed; 3 Perverted Under these, a. Conditions; b. Concomitance

2. *Memory.* As under Faculties.

30. DISPOSITION. 1. Quality; 2. Conditions; 3. Concomitance.

APPENDIX.

Annotations on the History of Homœopathic Literature in the English Language. Letter from Dr. Lippe.

Dr. Carroll Dunham in his excellent article "Homœopathy the Science of Therapeutics," published in the American Homœopathic Review, after complaining that it is humiliating to be compelled to say that there are no trustworthy manuals of our Materia Medica in the English language, alludes to the Preface of *Jahr's New Manual* and *Symptomen Codex*, by Dr. Constantine Hering, and refers to the subsequent publication of that Preface in the original German manuscript, in the *Allg. Hom. Zeitung*, and which contains also the following foot-note by Dr. Hering: "After comparison of the translation with the original, the above indorsement is hereby altogether and completely withdrawn." Dr. Dunham further says that if he were asked why this retraction was not made in the language and in the country in which the commendation was suffered to be published, he would have nothing to say. Dr. Dunham further continues: "It avails not to say, why find fault with these translations and this manual, inasmuch as we have no others? Had the unworthiness of these been made known, had they not, on the contrary, been indorsed by high authority, we had long since had others and trustworthy. An exposure of the imperfections we have spoken of would have created a demand for other works, and it is not less true in science than in trade that 'demand creates a supply.'"

Dr. D. Wilson, who is now publishing an article in the *Monthly Homœopathic Review*, beginning in No. 7, vol. vi., headed "How far is Dr. Hempel to be trusted as a translator of Hahnemann's works," says, in that number: "Yet, strange to say, no one, as far as I am aware, has hitherto publicly pointed out the blunders that have been perpetrated by Dr. Hempel in his voluminous translations of Hahnemann's works." To correct the above erroneous statements allow me then to publish the following facts:

Jahr's New Manual and *Symptomen Codex*, by Dr. C. J. Hempel, vol. iii., was reviewed by me for the *Philadelphia Journal of Homœopathy*, in 1852, in vol. i., page 427 to 432, my name not appearing. Dr. Hempel puts in a demurrer in vol. i., page 467 to 476 of the same Journal, dated December 15th, 1852. In vol. i., page 518 to 521 of that Journal, I felt compelled as the Author of the Review to answer, and there say: "I write (this criticism) for the *American Homœopathists*, to vindicate ourselves before the world," etc. Dr. Hempel answers again, February 24th, 1853, vol. i., page 549, and remarks that the interest of the publisher must be protected; he then appeals to the recommendations of Dr. C. Hering in the preface to the first volume of the

work, to which I replied, March 24th, 1853, vol. ii., page 59 to 61, vindicating Dr. Hering by stating that he could not then publish his original German preface or its correct translation, nor could he withdraw his recommendation in the country in which that commendation was published, as the *Homœopathic Journal* then published was in the hands of the publisher of the *New Manual*, who would not receive and circulate communications adverse to his interests. I further then gave there the foot-note of Dr. Hering in the *Allg. Hom. Zeitung*, above referred to. "After comparing the translation with the original, I solemnly withdraw this n[illegible] above communication and indorsement."

Dr. Hempel again replied, in vol. ii., pages 318 and 319, declaring me to stand *convicted* of libel, threatening legal proceedings or demanding an arbitration by committee. The committee report of the 24th of September and the 8th of October, 1853, is published in vol. ii., page 430, and this committee, having examined Dr. C. Hering's original German manuscript of the preface to the *New Manual* and its publication in the *Allg. Hom. Zeitung*, with the translation by Dr. Hempel as published, find, that the point assumed by me, viz., that the translation of Dr. C. Hering's preface to the *New Manual and Symptomen Codex* contained willful perversions and omissions, has been established. A reply to this report by Dr. C. Hempel is published in vol. ii., page 573, in which he boldly denies that he is guilty of any essential alterations. Later the publisher succeeded in obtaining by some means the names of a few physicians as a recommendation to the Complete Repertory.

The Review was published ten years ago. It had only the effect to procure for Dr. C. J. Hempel a professorship in the Pennsylvania College of Homœopathy. What effect his teachings and publications had while Professor in that Institute are well known.

That ten years later Dr. Wilson, in England, and Dr. Dunham, in the United States, should indorse my review of 1852 is exceedingly gratifying to me, but unfortunately does not remedy the evil done to Homœopathy and its progress during that length of time.

I sincerely hope that Drs. Wilson and Dunham will be more successful in 1862 in arousing the English-reading members of the profession to the necessity of accurate translations and correct versions of Hahnemann's writings and our standard works.

AD. LIPPE, M. D.

Philadelphia, December 7th, 1862.

NOTE.—The facts respecting the publications in the *Philadelphia Journal*, in 1852-53, are essentially as above stated by Dr. Lippe. The reasons why they did not attract general attention and were not accepted by the profession as a withdrawal *by Dr. Hering* of his strong indorsement of Dr. Hempel's translation of the Symptomen Codex, I suppose to be the following:

1. The first paper by Dr. Lippe was anonymous, and therefore was not authoritative in any personal sense.

2. The foot-note attached by Dr. Hering to the German publication of his preface was not quoted by Dr. Lippe until after a succession of demurrers by Dr. Hempel and replications by himself had given the matter the aspect of a somewhat personal controversy; and, even then it was introduced as a kind

of *obiter dictum*, because the foot-note referred to the "Symptomen Codex or New Manual," in two volumes, whereas Dr. Lippe's review had treated almost exclusively of the *Repertory*, or vol iii.

Now, this repertory was really to all intents and purposes not so much a translation as a new work by Dr. Hempel, and he is entitled to commendation or adverse criticism according to the merits or demerits of the work, as an Author rather than as a Translator. In such a capacity Dr. Hering's foot-note and preface could hardly refer to him, and hence the foot-note, introduced "by the way," in Dr Lippe's letters concerning his review of the Repertory failed to attract general attention and to vindicate Dr Hering.

But the above "annotations" are entirely satisfactory. Coming, as they do, from a confidential friend of Dr Hering, and with his express sanction and approval, they are a complete withdrawal of his indorsement of the English version of Jahr's New Manual They give, moreover, a satisfactory reason why this withdrawal could not have been published at an earlier date in this country, viz the fact that the publishing interest controlled all of our Journals from the year 1850 to 1858, to such an extent that nothing in the shape of an adverse criticism of any important publication was allowed to appear in any Journal. This statement the writer can corroborate from personal experience

While accepting Dr Lippe's annotations as full and satisfactory on these points, I am not disposed to adopt unreservedly his criticisms of Dr Hempel I have no evidence that the errors and omissions which render it impossible to receive his works, as trustworthy translations of the books they profess to represent are willful or malicious I think they may be all accounted for by the fact that the translations were made in haste and therefore carelessly, and that to some of the tasks which he undertook Dr Hempel may have been, at the time hardly competent. And when we consider the immense amount of labor required by these num rous translations, we can hardly wonder that errors have been committed and omissions made Unhappily, this does not make the books any less undeserving of confidence Of course an error or an omission is just as fatal to the trustworthiness of a translation, whether it be the result of haste of carelessness, or of willful perversion—but the interests of science do not require the critic to go beyond a statement of the fact that the work is not trustworthy, and of the particulars in which it fails

In my remarks on Manuals to which Dr Lippe alludes, I refer only to that part of the New Manual which relates to the Materia Medica The faults of the English version of the preface, however provoking to the Author of the preface, have not the same kind of relation to the general interests of the science. They do, however, concern the personal and professional reputation of Dr. Hering, and at his request we publish a letter addressed by him to Dr. Gardiner, Editor of the *Philadelphia Journal of Homœopathy* at the time of the publication of Dr. Lippe's Review of the Repertory before alluded to.

DUNHAM.

[COPY]

PHILADELPHIA, August 18th, 1853.

DR. WM. GARDINER,

Sir · You say, in your Journal, the last number, page 318, you "wish to deal justly"—"nothing further to be published on that subject." On the same

page you allow your own name to be put at the head of a committee to examine the original manuscript of Dr. Hering's preface, or as an alternative the mean threat of setting New-York pettifoggers upon the first one who has had ability and courage to awake the attention of American Homoeopaths to the danger to which monopolizing and book manufacturing leads our cause. I am willing to lay in the hands of any respectable committee meeting in Philadelphia, at any time,

1. My original manuscript.

2. The verbatim printed copy in the *Allg. Hom. Zeitung.*

3. The English translation of it as a preface to the Symptomen Codex.

4. A comparison of the translation with the original, pointing out the willful perversions and omissions.

Said committee, according to common sense and custom, cannot have any interested parties members of it, but ought to be appointed by them, and of course only of such physicians as understand the German language. You ought to be the umpire. Is not your Journal the proper organ in which such a report of such a committee ought to be published?

(Signed)

Yours,

CONSTANTINE HERING.

ANTAGONISM BETWEEN HOMŒOPATHY AND ALLOPATHY.

Address, delivered at the semi-annual meeting of the Homœopathic Medical Society of the State of New-York, Albany, February 10, 1863.

Gentlemen of the Society:

By the request of your president and other officers, I appear before you to perform that duty which, on such an occasion as this, devolves upon the president of the Society, but which the special engagements of that officer, at this season of the year, in New-York, have prevented his fulfilling.

The session of this evening brings to a close the first regular meeting of the Homœopathic Medical Society of the State of New-York.

The object of this Society is declared to be "the advancement of the science of medicine."

In these days, when the value of associated labor is so well understood, one might certainly ask, with surprise, "Can it be that, prior to this year, there has existed in the State of New-York no central organization for the advancement of the science of medicine?"

The fact is, there *has* been a State Medical Society in active operation since 1806. Its *object* is the same as that of our Society; its organization and its mode of operation are identical with ours.

What, then, is the necessity for a second Society? Why should men of the same profession, engaged in similar labors, for a common object, divide their forces, and thereby diminish their efficiency? What is the nature of the *antagonism* which this division implies, and what is the

necessity for its perpetuation? Candid and exhaustive replies to these questions will explain and justify our position of separation from the Old School of medicine. They will, at the same time, sharply define the outlines of that branch of medical science to which we have especially devoted ourselves, and will give us a clear view of the labors which devolve upon us for its advancement and development.

I propose, therefore, to discuss this antagonism,—first from a historic and then from a philosophical point of view.

Samuel Hahnemann, the great reformer of medicine, was a regularly educated physician, of great learning and very uncommon general culture and literary attainments. In the words of Sir John Forbes, who surely cannot be accused of any partiality for the founder of Homœopathy: "No candid observer of his actions, or candid reader of his writings, can hesitate for a moment to admit that he was a very extraordinary man,—one whose name will descend to posterity as the exclusive excogitator and founder of an original system of medicine, as ingenious as many that preceded it, and destined, probably, to be the remote, if not the immediate, cause of more important fundamental changes in the practice of the healing art than have resulted from any promulgated since the days of Galen himself; * * * he was undoubtedly a man of genius, and a scholar; a man of indefatigable industry and of dauntless energy."[1]

Hufeland, the Nestor of orthodox medicine in Germany, in calling attention to an essay published by Hahnemann, in his *Journal*, in 1801, speaks of him as "one of the most distinguished physicians in Germany."

This being the estimate in which Hahnemann was held by his most distinguished contemporary (Hufeland) and by his most learned critic (Forbes), both of whom, be it observed, were opposed to the medical reform which he had instituted, let us glance at his professional career.

[1] *British and Foreign Medical Review*, xli., 1846.

After practicing in various localities and positions, with such success and acceptance as to acquire the reputation which Hufeland records of being "one of the most distinguished physicians in Germany," Hahnemann tells the profession, in several essays on medical subjects, that he has become so deeply convinced of the uncertainty of medical practice, and of the positive injurious effects of many methods in common use among physicians at that day, that at length he really "doubts whether his patients would not, in many cases, have thriven as well, or better, *without* his aid as *with* it"

This conviction of the uncertainty of medicine, this suspicion of the injury which it sometimes inflicts on the patients, were not peculiar to Hahnemann Girtanner and several others, before his day, expressed them Sir John Forbes, from whom we have already quoted, says, in 1846, of the medical methods of our own time, "In a considerable proportion of diseases it would fare as well, or better, with patients, in the actual condition of the medical art, as more generally practiced, if all remedies, at least all active remedies, especially drugs, were abandoned" "Things [in medicine] have arrived at such a pitch, that they cannot be worse; they must mend or end"[1] Such views have been repeatedly expressed by members of the medical profession in this country.

Hahnemann has said nothing more severe nor more sweeping than this condemnation of practical medicine, by the late head of the profession in England.

But what did Hahnemann do when he had become convinced of the inutility and mischievousness of the current medical methods? Did he continue a routine practice for the sake of "making a living?" No! like a noble, honest man, he refused to make a pretense of curing where he *believed* he did *not* cure. He relinquished the practice of medicine and devoted himself to the collateral science of

[1] *British and Foreign Medical Review*, xli., 1846.

chemistry and to literary labors. But his mind was ever at work on the great question of the improvement of the practice of medicine, for he was "sure that the Creator had not left His creatures without a means of succor from the pangs and ravages of disease."

Thus intent on this subject, he could not fail to remark that although the prevailing treatment of diseases was, in general, blind and at least *ineffectual* to cure, yet there were certain remedies which were used in the case of certain diseases with almost uniformly happy results—or at least with such results as left no room for doubting that in *these* cases, at least, *real* cures were effected. This he had observed to be the result of the use of mercury in certain cases, not unfrequently encountered by medical men; but his attention was especially called to the fact in connection with Peruvian bark, the febrifuge properties of which had, during the latter part of the preceding century, become well established and highly prized on the continent.

"If," he thought to himself, "if the number of these specific remedies could be vastly increased, and if some system could be discovered in accordance with which we could ascertain their exact properties and could know beforehand in what cases of disease they would be applicable, then indeed would the uncertainties of medical practice be removed, then might we anticipate as great success in the treatment of all diseases as we now attain in the treatment of a few for which we have specifics."

This desire for specifics was not original with Hahnemann. It had been expressed before his day by Bacon and by Boyle. Sydenham had longed for them in expressions almost pathetic in their hopelessness. But Hahnemann, with his "dauntless energy and indefatigable industry," went to work to *discover* this system.

A casual observation in Cullen's Materia Medica gave him the clue to his discovery, as the falling apple did to Newton, and the swinging chandelier in the church at Pisa, to Galileo.

From this observation it occurred to him that provings of drugs upon healthy persons might furnish a knowledge of their specific properties, and that the administration of drugs in cases presenting symptoms similar to those which the drug produces in the healthy subject, might be the law of the application of specifics.

He sought throughout the whole medical literature of ancient and modern times for instances bearing upon this subject, and he collected a large mass of evidence corroborating his speculations.

He then proceeded to verify his theory by actual experiment. First upon himself and then upon all healthy persons who would join him in these self-sacrificing labors, he proved the effects of a number of drugs. Then, cautiously, first in his own family, and then, little by little, in his general practice, which he had now resumed, he gave, as occasion offered, the drugs which he had proved, in cases of disease that presented symptoms similar to those produced by the drugs.

From 1790 to 1805, fifteen years of the prime of his life were devoted to constant, exhausting labors of this nature, during which time he proved on his own person more than sixty drugs; "for," said he, "when we have to do with an art whose end is the saving of human life, any neglect to make ourselves masters of it becomes a crime!"[1] At the end of this period, sure of the truth of the great principle he had discovered,—with all the incidental testimony of history to support it—with the positive results of a long experience to confirm it,—he presented his views and the results of his labors to the profession in an essay of wonderful logical power, of the utmost moderation in expression, full of almost tender persuasion and of the noblest enthusiasm;[2] and he published at the same time the first part of his Materia Medica.[3] Five years later appeared the more elaborate exposition, the "Organon."

[1] Dudgeon, Hahnemann's Lesser Writings.

[2] Medicine of Experience.

[3] Frag. de Vir. Med. pos.

This was the turning-point of Hahnemann's career. Let us see what was his relation to the profession at this time.

He had, by universal consent, attained a position in the profession which justified him in assuming to criticise prevailing methods and to suggest improvements. He had shown the need of improvements, and he had borne testimony to his honesty in this exposition, by retiring from a lucrative practice. He now came before the profession saying, "I believe I have discovered a system which will render the practice of medicine certain, and its success brilliant. I have labored fifteen years to test my discovery. My own experiments and the testimony furnished by the records of medicine convince me of its truth. I lay it and them before you, my colleagues, and I conjure you in the name of truth, by the interests of humanity, to investigate it candidly and without prejudice." "If," he says in his letter to Hufeland on this occasion, "if experience should show you that my method is the best, then make use of it for the benefit of mankind, and give God the glory!"

How were this exposition and appeal received by the medical men of the day?

In 1811, appeared the Anti-Organon of Prof. Hecker—a work full of the most bitter aspersions upon Hahnemann's personal character, whereas, in fact, the question had relation to *principles* and not to *persons*; abounding in the most concentrated contempt and scorn of the system which Hahnemann had unfolded; and without a single suggestion to investigate, by *practical* experiment, the *practical* method which Hahnemann had stated to have been attended, in his hands, with such brilliant *practical success!*

And from that day to the present, all the utterances of the Old School, whether from the press, the council, the professor's chair, or in the forum of the academy, have been bitter personal denunciations and aspersions of the character and motives of Hahnemann, and of all who have adopted or have even shown a disposition to investigate his method.

But many a scientific discoverer has met with opposition and calumny at the hands of his colleagues. Not to go beyond the ranks of medicine, Harvey was denounced as a quack, because he demonstrated the circulation of the blood! Jenner was scandalized with most persistent violence because he introduced vaccination.

To Hahnemann, however, persecution came nearer home. After he had satisfied himself of the value of his discovery of the true method of medical practice, he resumed the exercise of his profession. His success was more brilliant than it had ever been. His fame as a practitioner grew rapidly, and patients began to come to him from considerable distances. This good fortune excited the envy of his colleagues in Königslutter, where he then resided. At their instigation, the apothecaries of the place brought a prosecution against him for infringement of the law which forbids to practitioners of medicine the compounding and sale of the remedies they prescribe. For, it must be observed, that, as an inevitable corollary to his new system of practice, Hahnemann had come to prescribe only a single drug at a time, and that he used simple preparations such as could not be obtained in the requisite purity at the apothecaries'. In vain it was urged that the spirit of the law was not infringed, since Hahnemann himself was an expert apothecary and chemist, and since his remedies were not "*compounded*," but *simples*, and not "sold," but dispensed gratuitously. The opposition was too strong. He was forbidden to practice, save in accordance with the law alluded to.

Rather than yield in a matter which he considered essential to the freedom of the physician and to the purity and certainty of his practice, Hahnemann determined to leave Konigslutter; and accordingly, to the delight of his colleagues and of the apothecaries, and to the regret of the citizens, who were loath to lose their benefactor, and a cortege of whom attended his carriage far beyond the gates of the town, he removed to Hamburg.

Here, as he became known and appreciated, the same persecution was revived, and with the same result. He removed to Altona.

In this way, during a period of twenty-two years, from 1799 to 1821, Hahnemann was constrained, by the persecution of his colleagues, under cover of the law, to change his abode at least eleven times. The last place from which he was driven in this manner was Leipsic—a city for which he had a peculiar affection. Here he had pursued his earliest medical studies and met with his first successes. Here he had, in later years, established a college of Homœopathy, and had lectured to large audiences. In the shady walks and groves that surround the city, he had been wont to spend the evening of each day in social converse with his family and with the students whom he had gathered about him and who took part in his labors of proving drugs.

From this city of his love, the scientific capital of his fatherland, he was now, in the sixty-sixth year of his laborious life, driven away to an asylum offered him in the tiny capital of the tiny Duchy of Anhalt-Coëthen!

No wonder that he who for so many years had followed the injunction, "When they persecute you in one city, flee ye to another,"—that he who, like the Divine Healer, had gone "about from place to place doing good and healing all manner of sickness and disease among the people"—no wonder that he forgot, under the pressure of this last indignity, that other injunction of the Divine Teacher, to "love them that hate you, and pray for them that despitefully use you and persecute you," and that, like Luther, he now bared his hitherto sheathed weapons of satire and invective against those who had striven to hinder his usefulness—who had so cruelly marred his peace and happiness—all save that peace which can never be taken from the man who has within himself the "*mens sibi conscia recti!*" If in this he erred and came short of the Divine

example, let him among men who is "pure," cast the first stone at him!

Time brings its sweet revenges. After a career of honor and usefulness at Coëthen, where his ever-increasing fame brought him throngs of patients from all parts of Europe, and a subsequent residence in Paris, where his reputation extorted from the government a license to practice as he pleased, Hahnemann died at Paris in 1843, full of years and honors. Eight years afterward, in 1851, the town council of Leipsic appropriated a beautiful plot of ground as a site for his monument, and the council celebrated officially the uncovering of a costly and beautiful bronze statue of that man, as one of Saxony's most illustrious sons, whom thirty years before the same council had ignominiously chased from their borders as an unauthorized and illegal prescriber!

Before we leave this branch of our subject, let us draw one lesson from the story of Hahnemann's persecutions. All his sufferings might have been avoided, he might have lived in peace and affluence, enjoying consideration among his colleagues and making *plenty of money*, had he been willing to "yield a little," to waive the right of dispensing his own medicines, to accommodate his system in various points to suit the notions of his time. The temptation to do this might, by some, be supposed to have been great, for Hahnemann's family was large, he suffered during his wanderings from the pinchings of cruel poverty, and this took from him the leisure so necessary for his studies.

But Hahnemann was not made of the stuff that could compromise, for personal ease and prosperity, the charter that God had given him for the benefit of the race. He refused to give up one particle of anything which he deemed essential to the purity and perfectness of his system, and so he has left it to us, pure and perfect!

Let us remember his example when prospects of ease and consideration, and of the cessation of strife for the truth,

tempt us to compromise unworthily with any portion of the Old School of medicine.

For there are those among us, as there are men in other walks of life, who, for the sake of what they call *peace* and *union*, would join hands with what they know to be false! aye, even though to do it, they should have to "cut off the fanatics," who adhere strictly to Hahnemann,—to leave the "*brains*" of their system "*out in the cold.*"

This is the origin and the personal history of the *antagonism* between Homœopathy and the dominant school of medicine. Hahnemann showed the imperfections of the current methods. Nobody disproved what he said. Everybody agreed with him and everybody sighed for something better. He *discovered* something better and offered it to his colleagues, with demonstrations of its value; he begged them to investigate it; and in case they should find it better than the old method, to use it for the good of mankind, and give God the glory! Then, with one accord, they denounced him as a vile impostor, and chased him from their midst, nor have they yet ceased to heap ignominy on his name!

It may be objected that I have not stated the whole grounds of the opposition of the Old School to Homœopathy, inasmuch as I have said nothing about the "little doses."

If this were true, it would not alter the bearings of the case, because the doctrine of the "little doses," like all the rest of Hahnemann's method, was offered to the profession, to be by them submitted to the test of experience, by which, like all the rest, it should stand or fall. But, in point of fact, Hahnemann came very slowly to see the necessity of giving small doses when he prescribed according to the law of Homœopathy; and he did not express himself authoritatively upon this subject until long after the opposition to him and the prosecutions in the name of the apothecaries

were in full blast! Therefore, this opposition could not have originated in the doctrine of the *dose*.

Nor is the question of the dose at all essential to the experiments which Hahnemann invited his professional brethren to make for the purpose of testing his system. Intelligent experiments with doses of ordinary size would convince any physician of the truth of the Homœopathic law; and if he continued the experiments, the inconveniences that he would find to result from the use of such doses would inevitably lead the experimenter, as they led Hahnemann, to continually diminish the dose, until he should become convinced of the truth of Hahnemann's dogma on this subject also

This has been the uniform experience of all physicians who have become convinced, through experiment, of the truth of Homœopathy, and have adopted the method in practice And the more strictly they conform to Hahnemann's method in prescribing, the more exactly do they agree with him respecting the dose. The number of these witnesses amounts to-day to many thousands, and their concurrent testimony does not admit of dispute.

Let us now consider this antagonism from a philosophical point of view.

As I have already said, Hahnemann perceived that the prevailing method of treating disease comprised two processes.

One of these processes was what was then, and is still, called the "Rational." It involves a theory of the cause and essential nature of the disease, and the resort to some expedient which would be likely to remove this supposed cause of the disease, and to bring about a contrary state, and so conduce to health. Of this kind was Galen's method, which divided diseases into *hot*, *cold*, *moist* and *dry;* made a similar classification of remedies, and applied to each disease a remedy from a class of the contrary

nature. Less glaringly absurd, but in no way different in nature, are the theories which hold that certain diseases, for example, are caused by accumulation of the blood in certain organs, and are to be cured by abstraction of blood; that others depend upon what is imagined to be *want of tone*, and are to be cured by remedies which are assumed to be *tonics*; that others are due to a languid state of the "*animal spirits*," and are to be encountered by the administration of "*stimulants*," etc.

That these were the grounds upon which the prevalent methods, in the generation preceding Hahnemann, were based, is shown by Cullen's theory of the action of bark, and also by the following passage from "Sydenham on Pleurisy:"

"After attentively considering the various phenomena of this disease, I think it is a fever originating in a proper and peculiar inflammation of the blood,—an inflammation by means of which nature deposits the peccant matter in the pleuræ. In my treatment, I have the following aim in view: to repress the inflammation of the blood, and to divert those inflamed particles which have made an onset on the lining membrane of the ribs (and which have lit up so much mischief) into their proper outlets. For this reason, my sheet-anchor is *blood-letting*."

A modification of this process is that which is known as the Hippocratic method of observing and following the indications of nature; in the words of Sydenham, "watching what method Nature might take, with the intention of subduing symptoms by following in her footsteps." According to this method, if, in any disease, recovery was preceded by a critical evacuation,—such as a copious sweat,—this was assumed to be nature's method, and sudorifics were accordingly resorted to in similar cases.

Now, independently of the fatal objection, that this method would confine our curative power to such diseases as Nature *herself* is wont to cure by critical discharges, etc.,—

the very diseases, therefore, in which medical aid could be best *dispensed* with,—while it makes no provision at all for such diseases as rarely or never get well of themselves,—such as nature never cures by critical discharges,—the very cases, therefore, in which there is *most* need of the intervention of art,—Sydenham tells us that he "found that spontaneous sweats often did good." "But these," he says, "were very different things from *forced ones*." And Hahnemann showed that "in such cases the critical discharges and the recovery were *simultaneous;* that the discharge was the *consequence* and *announcement* rather than the *cause* of the *recovery;* and that to infer from such a state of things that we could bring about a cure by inducing an *artificial* sweat, would be like ringing bells and lighting bonfires to *secure* a *victory* instead of to *announce* one."[1]

The other process, which Hahnemann perceived to be comprised in the prevailing methods of treatment, was—administering, in the case of a very few diseases, of which fever from marsh miasma may be taken as the most illustrious example, certain remedies which had been discovered to possess, in some unexplained way, a power to cure these diseases. Such remedies had been discovered by the merest accident. No method was known by which others could be discovered: no method had been suggested by which it could be more clearly ascertained and defined for what particular varieties of the diseases in question these "specifics," as they were called, were especially appropriate.

Hahnemann showed the fallacy of the philosophy on which the "Rational" method was based. He showed that, even admitting that accumulations of blood do exist as the "proximate cause" in certain diseased conditions, yet these accumulations cannot be regarded as the *essential cause* of the disease. On the contrary, that cause must be sought in that force which regulates the circulation and preserves its equilibrium. This force must have been set into abnormal

[1] Russell, *History and Heroes of the Art of Medicine.*—"Sydenham."

action, in order that an accumulation could take place in any particular organs; and, therefore, in the accumulations we see, not the *essential cause* of the disease, but only one of the *results* of that *cause*. To undertake, then, by abstraction of blood to remove a result of the *cause*, is not to *cure* the disease by removing the *cause*, but only to seek to *palliate* it by carrying away some of its *products*. He showed, further, that this abnormal condition of one of the dynamic forces, the action of which constitutes the life of the body, is beyond the sphere of our observation. Like the healthy action of these same forces—in a word, like life—it is an ultimate fact, behind which we cannot penetrate, and which, therefore, we cannot study as a *cause* of disease and seek to remove by direct rational means.

But Hahnemann went farther, and showed that, although we cannot investigate the ultimate nature and causes of those modifications of the dynamic forces of the organism, which are the essential causes of diseased action, and then remove them, as a "rational" method would propose to do, yet that if, while a disease is in full vigor, we administer a remedy which causes a sudden cessation of the morbid action without any abstraction of the fluids or any derivative action whatever, we are then justified in concluding that the remedy we have given has, in some way or other peculiar to itself, reached that force which was in a state of abnormal action, and has so modified it as to bring it back to a condition of normal action; that this remedy has a "specific" effect upon that force under certain conditions; and we draw this conclusion upon the general principle that when an *effect* ceases we may conclude that the *cause* has ceased to act.

And Hahnemann showed, farther, that if we could discover substances having such a specific action, and a law by which we should know just when to apply them, we should have accomplished the much needed reform in medical science.

He appreciated so highly the value of the *specifics* of which medicine was already in possession, that he consecrated his

life to the task of discovering a method of increasing their number and of reducing their use to a system.

In this appreciation of the direction in which alone improvement in the curing of diseases was to be looked for, Hahnemann was anticipated, as I have said, by Bacon, Boyle and Sydenham.

Thus Bacon, in the "Advancement of Learning," after a sweeping condemnation of the unphilosophical method of Galen, says: "A work is wanting upon the cures of reputedly incurable diseases, that physicians of eminence and resolution may be excited and encouraged to pursue the matter as far as the nature of things will permit; since to pronounce diseases to be incurable is to exhibit ignorance and carelessness, as it were, and to screen ignorance from reproach." And again, "I find a deficiency in the receipts of propriety respecting the cure of particular diseases." Again, "They have no particular medicines which, by a specific property, are adapted to particular diseases. I remember a learned Jew physician who used to say, 'Your European physicians are like bishops; they have the keys of binding and loosing—nothing more!' It would be of great consequence if physicians eminent for learning and practical skill would compile a work of approved and experienced medicines in particular diseases."

The learned Boyle, the father of chemistry, who had devoted much time to the study of medicine, says: "I cannot forbear to wish that divers learned physicians were more concerned than they seem to be to advance the *curative* part of their profession, without which, three at least, of four other parts, may prove indeed delightful and beneficial to the *physician*, but will be of very little use to the *patient*, whose relief is yet the *principal end of physic*. I had much rather that the phvsician of any friend of mine should keep his patient, by powerful medicines, from dying, than tell me punctually *when* he shall die, or show me in the opened carcass *why*, it is supposed, he lived no longer."

Again he says, when speaking of the need of specifics: "Finding at every turn that the main thing which does prevail with learned physicians to reject specifics is, that they cannot conceive the distinct manner of the specifics' working, and think it utterly improbable that such a medicine, which must pass through digestions in the body and be whirled about by the mass of the blood to all the parts, should, respecting the rest, show itself friendly to the brain, for instance, or the kidneys, or fall upon this or that juice or humor rather than any other.

"First, I would demand of these objectors a clear and satisfactory, or at least an intelligible explication, of the manner of working of divers other medicaments that do not pass for specifics. For I confess that to me, even many of the vulgar operations of common drugs seem not to have been hitherto intelligibly explained by physicians, who have yet, for aught I have observed, to seek for an account of the manner of how diuretics, sudorifics, etc., perform their operations, etc.

"The same objection that is urged to prove that a specific cannot befriend the kidneys, for example, or the throat rather than any other parts of the body, lies against the obnoxiousness of poisons to this or that determinate part; yet experience manifests that some poisons do respect particular parts of the body without equally or at all sensibly offending the rest; and we see that cantharides, in a certain dose, are noxious to the kidneys and bladder, and quicksilver to the throat and glandules thereabout, stramonium to the brain and opium to the animal spirits and *genus nervosum*."[1]

Sydenham expresses himself on this subject with his accustomed brevity and directness. Speaking of intermittent fever, he says: "We must do one of two things; we must, by careful and anxious observation of *the process by which nature relieves herself of this disease*, draw indications as to the manner by which the incipient fermentation may be promoted

[1] Russell, *History and Heroes of the Art of Medicine*, vol. ii., p. 101; art. "Robert Boyle."

and the patient restored to health, or else we must *discover a specific*. By the latter method, we shall attack the malady directly."

It may be observed that Sydenham did not hesitate to choose the latter method so soon as the specific virtues of Peruvian bark in intermittent fever were recognized, and that he was the chief means of making the value of this great specific known in England; nor have the profession generally, since his day, been disposed to hesitate in their choice between the Hippocratic and the specific methods of treating this disease,—the two alternatives which Sydenham so clearly lays down.

He continues: "By no means do I wish to express myself as if wise and learned physicians were to despair; as if they were to think out no better modes of treatment, and as if they were to throw away the hope of discovering nobler and more potent medicines for accelerating the cure of diseases. So far am I from this that I do not despair of finding out, even myself, some *such medicines* and some such *method of curing*."

These things which Bacon, Boyle and Sydenham point out so forcibly as the *desiderata* of Medicine, which Sydenham did not despair of finding out, yet died without discovering,—these "specific medicines," and this "*methodus medendi*," were offered to the profession one hundred years later by Hahnemann.

Dr. Lettsom tells us, "the great Sydenham, for all his labors, only gained the sad and unjust recompense of calumny and ignominy, and that from emulation of some of his collegiate brethren and others, whose indignation at length arose to that height that they endeavored to banish him from that illustrious society (the Royal College of Physicians), as guilty of medicinal heresy."

And yet Sydenham only longed for, and looked forward to, the discovery of specifics and of the law of their employment. He was the Moses of the specific method. It was,

therefore, in the regular course of historic sequence that Hahnemann, the Joshua of that method, who led the hosts of Æsculapius into the promised land of which the Moses had had a glorious vision, should be unsparingly denounced as a heretic and actually banished from every well-regulated society!

This, then, is the *antagonism*. Hahnemann shows that specifics are to be discovered by ascertaining the effects of drugs upon healthy persons; that they are to be applied by giving to a sick person such a drug as would produce, in the healthy subject, symptoms similar to those of the sick person. He presents this discovery to the profession as something in advance of present knowledge. They refuse to accept or even to test it, and they denounce him for offering it. On which side lies the *onus* of the antagonism?

But it may be said, however true these statements are as regards the age for which Hahnemann wrote, the scientific progress of the last fifty years has changed all that. It *has* changed the *names* of things and little besides in therapeutics. We hear no more, it is true, of "temperaments" and "humors," of the "animal spirits," of the "*Arcæhus*," but instead, the talk is now of "the dyscrasias," of "diatheses," of the "cellular pathology," of "analogies" and "heterologies."

There is the same endeavor to draw from a theory of the *essential nature* of the disease a rational indication of cure, of which Hahnemann exposed the fallacy and impossibility. Indeed, Sir John Forbes affirms, in 1846, "The progress of therapeutics (the cure of diseases) during all the centuries that have elapsed since the days of Hippocrates, has been less than that achieved by the elementary sciences of medicine during the last fifty years. This department of medicine must indeed be regarded as yet in its merest infancy."

It should be clearly understood, and I state it most emphatically, that all expositions of the insufficiency and

the chaotic state of the prevalent system of medicine—whether by the outspoken leaders of the Old School, like Forbes, or by Hähnemann and his followers—refer exclusively to the department of therapeutics, the science and art of curing diseases by medicines. In the development of the natural history of the healthy and of the diseased body, that is to say, in the sciences of physiology and physiological anatomy, and of pathology and pathological anatomy, as well as in the departments of hygiene, surgery, obstetrics and medical chemistry, medicine has fully kept pace with the wonderful progress of scientific knowledge in our day. We profit by the labors of our colleagues in these branches, and accord them full recognition and admiration. But the great *end* and *object* of all these things is to *cure* diseases. If they afford no facilities for this, they are profitless to mankind Now, if the same men who have brought these collateral sciences to such perfection have been unable to bring therapeutics out of what Forbes calls its present chaos of "merest infancy," is not the conclusion irresistible that they have not yet got hold of the right clue—of the true philosophy of the science?

There is even a greater indisposition in our day, than in the time of Boyle, to admit the value and to stimulate the discovery of specifics. Nor is this wonderful; for specifics obstinately refuse to range themselves under any rational hypothesis. They exert a peculiar, inscrutable action upon certain organs when in certain conditions, and more than this nobody can say of them. And yet, notwithstanding their philosophical aversion to them, the practical sagacity of our colleagues, with which they keep their philosophy strangely at variance, leads them to seize eagerly upon specifics whenever these are presented to their notice. Witness the avidity with which they have availed themselves of Iodine, Ergot, Cod Liver Oil, the Hypophosphites, Iron, Veratrum viride, as well as of Nickel and Oxalate of Cerium which the Houdin of orthodox medicine has lately intro-

duced to them. Nay, people do say that our learned friends of the Old School make frequent use, "upon the sly," of Aconite as a specific in *fever*, and of Nux vomica and of Pulsatilla as specifics in gastrodynia and dysmenorrhœa, etc., etc.,—remedies of the specific properties of which their only knowledge is derived from the labors of Hahnemann.

It is true, they undertake to give a "rational" theory of the action of these specific remedies, but with as little success as Cullen met with when he attempted to explain the febrifuge action of Peruvian bark. It is all comprised in the doggerel explication that Molière gives of the hypnotic effects of opium:

"Domandatur causam et rationem quare
Opium facit dormire.
A quoi respondeo,
Quia est in eo
Virtus dormitiva,
Cujus est natura
Sensus assoupire."

Now, as in the days of Hahnemann, there is an *antagonism* between the Homœopathists and the Old School. The former hold out to the latter what they believe to be that method which has ever been a desideratum in medicine. The latter refuse even to examine it, and expel the Homœopathists from all associations over which they hold control. We cannot unite with them in any associated labors without ignoring and disavowing what we believe to be the true theory and practice of the all-important part of medical science—the science of therapeutics. *They will not unite with us* in associated labors for the development of this science.

Had Hahnemann done nothing more than devise a method of discovering and using specifics in the gross and wholesale sense in which the term was and is understood and used by the Old School of medicine, he would still have been entitled to the gratitude of mankind. He did far more.

He perceived that, in the dominant school, a specific was set apart as adapted to any individual member, indiscriminately, of a large nosological group [of diseases]; that bark, for example, was held to be the specific for malarious fevers in general, and that no account was taken of individual *deviations* within the limits of this group. Whereas, in point of fact, he perceived what is well known within the profession and without it, that although bark is really the specific for many, indeed for the majority of malarious fevers, it is not so for *all;* since many cases cannot be cured or even improved by its use.

Now, this wholesale way of regarding diseases, in *groups*, was the logical and inevitable consequence of the Old School theories of disease and of the method of cure. When Galen propounded the doctrine that all diseases depend upon one of four conditions, heat, cold, dryness, moisture, it was an inevitable consequence that he should disregard every phenomenon presented by his patients, except such as served to indicate that the case belonged to one of the above categories, and that he should have but four classes of remedial agents appropriate to these categories.

And when, in our day, physicians assume to ascribe diseases to certain pathologico-anatomical states as their essential causes, it is manifest that whatever varieties the case of each one of a dozen patients may present, must be disregarded, provided the pathologico-anatomical condition be the same in all, for they are grouped on the *basis* of this condition, and the indication for the cure is drawn from the *existence* of this condition, and must *necessarily* be the same for all.

Now, just as any spot upon the surface of the globe may be approached by an almost infinite number of roads, and yet, when the traveler has reached the spot, there shall be nothing in the mere fact of his presence there to indicate with certainty the road by which he has come thither, so the same *pathologico-anatomical result* may issue from the most

multifarious *pathological processes*, which processes, however, leave no sign in the *result*. If, then, the mode of treatment be based on the *result*, it can take no account of this variety in the *processes*.

A wholesale generalization, then, of diseases and of remedies is inevitable from the philosophy of the "Rational" method.

The common experience of the community teaches men that diseases to which the same name is given may present in different persons an entirely different aspect. John Doe and Richard Roe both have rheumatism, but their symptoms and whole condition are so entirely different that no one would have imagined them to have the same disease, if the doctor had not said so. The pathologico-anatomical condition, however, is the same in both (viz., the altered condition of the blood), and consequently the rational indications for the cure are identical in the two cases. If the doctor be true to the philosophy of his method, he treats them alike, notwithstanding the difference in their symptoms and apparent condition. But I call your attention to the fact which is familiar to every one, that every sagacious and long-headed physician of the Old School pays the less regard to the rules of his art the more experience he acquires at the bedside.

In treating disease he "feels his way," as the saying is; he relies on his "practical tact and experience," and often deviates widely from the rules of practice as they are deduced in the books from the theory of the art; he trusts less and less implicitly to a pathologico-anatomical basis of treatment, and more and more to "general indications," by which he means the sum of the *symptoms* of each individual case. In truth, experience has taught him the fallacy of the science of therapeutics, as founded on the "Rational" basis.

Now, if this were a *true* science, would not experience rather *confirm* the practitioner's faith in it, and add to his skill in applying it?

When on the other hand we cast aside all endeavors to base a method of treatment upon a theory of disease; when we give over all attempts to discover the inscrutable, essential nature and cause of diseases, and confine our observations to the phenomena of morbid action, whether these be *material* or *functional*, then we can take into account the *pathological processes* as well as the pathologico-anatomical *results*. We are *then* in condition to give due weight to the peculiarities of each individual case of disease, to study *it*, as under other circumstances groups are studied, and to give due attention to the modifying *idiosyncrasies* of the *individual*.

Hence, under our method alone is an absolute individualization of disease possible. Such a method of studying disease, however, would be barren under the Old School method of treatment, even with *specifics*. For the properties of specifics were known to the Old School only with reference to large and ill-defined *groups* of diseases. By us, on the other hand, specifics are studied in the effects which they produce upon the healthy subject, precisely as diseases are studied in the effects which morbific causes produce upon the sick. The same strict individualization, then, is practicable with regard to specifics that we have seen to be necessary in the study of diseases. It is not only practicable—it is fruitful of the *richest returns*.

The case, therefore, is not half stated when we say that Hahnemann discovered the *method of specifics*. He taught us *how* to discover and apply, and showed us the *necessity* of applying, an *individual specific* to each individual case of disease, as studied in the totality of its phenomena and without regard to the nosological group to which, for purposes of classification, the case might be assignable.

As a necessary consequence of this individualization, Hahnemann taught the value of *subjective symptoms*. By these, we mean those symptoms of which the physician becomes cognizant through the sensibilities of the patient. Among

them are all the varieties of pains and abnormal sensations which accompany disease. These symptoms were previously disregarded and are still considered as of no value by the "rational" or "physiological" school of medicine. Thus, Professor Bock, of Leipsic, in his work on Diagnosis (1853), says: "Only the *objective* symptoms—of which the practitioner derives a knowledge by the use of his own five senses, by sight, touch, hearing, mensuration, percussion, and by microscopic and chemical examinations—are of any value to the physician. The *subjective* symptoms are in *the highest degree uncertain* and treacherous." In other words, the disease is to be studied in all cases just as the phenomena of an inanimate plant or mineral are studied; the case of an intelligent and self-possessed patient, just as that of a patient whose intelligence is dormant under the cloud of a typhoid fever!

Now, everybody knows that pains and various sensations different from those of health, make up a large and important part in every case of illness. And what are these sensations? Unquestionably they are the results of abnormal action of the sentient nerves, or else they are evidences conveyed by the sentient nerves of abnormal action in some of the organs of the body.

It is in the highest degree unphilosophical to arbitrarily disregard and cast aside any of the phenomena of disease. If they find no place in our system of medicine, why, so much the worse for our system! If they cannot be made available under our method of treating disease, this fact is *prima facie* evidence that our method is defective! Furthermore, every physician and every intelligent person knows, by observation and experience, that pains and abnormal sensations almost always precede any material or organic evidence of disease. Common sense teaches every man the value, in medical treatment, of the maxim, "*Obsta principiis!*" Diseases should come under treatment at the earliest possible moment. Some that are curable at an

early period are well known to be incurable by our present resources, if allowed to establish themselves firmly. But the first evidences of nearly all diseases consist of *subjective symptoms*. A method, therefore, which does not provide for the employment of these symptoms in the determination of the treatment, cannot furnish means for encountering disease at the very outset.

And, appealing again to the experience of the community to bear me out, I say that honest and candid practitioners of the Old School often say to their patients, "Wait a little until your disease shall have become developed,—at present I know not what to do." Why does he not know what to do? This question touches the weak point in the philosophy of the physiological school. It is because the patient, as yet, presents only "subjective symptoms," which are evidence of "dynamic changes" only,—because he cannot form a theory of the *cause of the disease* until the disease has progressed far enough to furnish him with some *material results* of these *dynamic changes*,—in a word, with *objective symptoms*. This instance is another evidence of the insufficiency of the Old School philosophy of medicine, while at the same time it shows that, so long as they accept this philosophy, the physiological school are consistent and logical in excluding subjective symptoms from consideration.

Not a few diseases—the neuroses, for example—consist almost entirely of subjective symptoms. In these, the physiological physician admits his inability to institute a rational treatment, and he resorts at once to specifics. But if the specific method of treatment is available against diseases for which the physiological method fails, as well as for all other cases, is it not confessedly the universal method?

Hahnemann's method, which avails itself of *all* the phenomena which the patient presents,—holding that all, together, make up the disease,—sets a true value upon subjective symptoms. Not requiring a theory of the *nature and cause* of the disease as a preliminary to the treatment,

but basing the treatment directly upon the phenomena which the patient presents, it can proceed to cure a patient who presents only *subjective* symptoms as readily and as surely as one in whom these have given place to *objective* symptoms.

More than this, Hahnemann showed the value of subjective symptoms in the aid which they afford us in individualizing cases of disease, and thereby enabling us to select a specific for each individual case with more absolute precision. Indeed, he showed that it is only by means of subjective symptoms that the application of individual specifics is *possible.* It has already been remarked that the material or organic changes in the tissues of the body, which furnish the *objective* symptoms, may have resulted from any one of a number of *pathological processes* or *abnormal alterations of function*, and that they give us in *themselves* no means of knowing from what particular abnormal process they resulted. Now, unless we know this, we cannot apply specifics with exactness. The physiological school have no means of knowing it, and therefore they, very logically, do not *undertake* to apply specifics with exactness to individualize cases of disease. But the *subjective* symptoms enable us to take cognizance of these pathological processes, these abnormal changes of function, and hence the value of these symptoms. To Hahnemann belongs the honor of having demonstrated this value, and of having shown us how to avail ourselves of it.

To Hahnemann, again, belongs the credit of having insisted upon the propriety of using only one remedy at a time. As this, however, was clearly expressed by Boyle, and is admitted by Sir John Forbes, I shall content myself with quoting their words. Boyle says (1654): "It seems a great impediment to the further discovery of the virtues of simples, to confound so many of them in compositions; for in a mixture of a great number of ingredients, it is hard to know what is the operation of each or any of them, so that

I fear there will scarce, in a long time, be any progress made in the discovery of the virtues of simple drugs, till they either be oftener employed singly or be but few of them employed in a single remedy." And Forbes says, in 1846: "Our system here is greatly and radically wrong. Our officinal formulæ are already most absurdly complex, and our fashion is to double and redouble the existing complexities. This system is a most serious impediment in the way of ascertaining the precise and peculiar powers (if any) of the individual drugs, and thus interferes in the most important manner with the progress of therapeutics."

And, finally, Hahnemann demonstrated these facts:

1st. That the *curative power* of a specific remedy is not in the *direct* ratio of its *material quantity*. This had been suspected by his predecessor Sydenham, with respect to bark.

2d. That a drug exerts a more powerful effect on those organs for which it has a specific affinity when these organs are *sick* than when they are *healthy*; whence it follows that *smaller doses* of the same drug are required to *cure* diseases for which it is the specific remedy, than would be needed to produce their symptoms in the healthy person.

3d. That not only are smaller doses of specifics required to *cure* diseases than to produce their symptoms in the healthy, but that, when the symptoms of the diseased organism are *similar* to those produced in the healthy subject by a given drug, then this drug will act curatively on that organism in doses *so* small that they would hardly produce any effect whatever on the healthy organism.

But *how* small? This is the practical question. *A priori*, Hahnemann said, "The smaller the better, provided they only cure the disease in the quickest and surest manner." He experimented for the purpose of reaching definite conclusions, and out of these facts and experiments came the doctrine and practice of the little doses. And I repeat that those who follow Hahnemann most closely, individualize their cases most strictly, and select the individual specific

with most exactness, will, surely arrive, as all such have done, at the conclusion, on this subject, to which Hahnemann came.

It is remarkable that the first and second facts I mentioned respecting the dose of specifics did not escape the acute mind of Robert Boyle. He says: "To show you that a distempered body is an engine disposed to receive alterations under such impressions as will make none upon a sound body, let me put you in mind that those subtle streams that wander through the air before considerable changes of weather disclose themselves, are wont to be painfully felt by many sickly persons, and more constantly by men that have had great bruises or wounds, in the parts that have been so hurt,—though neither are healthy men at all incommoded thereby, nor do those themselves that have been hurt feel anything in their sound parts whose tone or texture has not been altered or enfeebled by outward violence."

If *quantity* be accepted as the measure of *power*, then the question of the dose must be resolved by the well-known laws of physics.

But so soon as it is admitted that the *power* of a drug is not determined by its *quantity* alone, but also by its form, and still more by the *condition of the patient* and by the relation of the specific properties of the drug to the diseased condition against which it is administered, then the whole question is withdrawn from the domain of *physics* and is left open to be settled by *experiment.*

Now, it is admitted by all medical men, that some drugs at least, exert their *specific* influence more speedily and more powerfully when given in *small doses* than when given in *large doses,* as, for example, Mercury, and according to Dr. Christison, Oxalic acid, which shows that there is not *always* a direct ratio between power and quantity.

"Moreover, all medical men agree that in certain morbid states, the body is much more sensitive to the specific action of certain remedies than it is, in certain other morbid states,

to the same remedies. The phrase that "such or such a disease does not bear Mercury well," is familiar to the professional ear. This shows that the condition of the patient has something to do in determining the power with which a certain dose of a specific shall manifest its action, and conversely that this condition should be taken into consideration in determining the dose.

These two facts show that the whole question of the dose was, at the time of Hahnemann's writing, an open one, to be settled by experiment alone.

And, as Hahnemann showed that those morbid conditions of the system in which a specific exerts the most power in small doses,—in which, in other words, the susceptibility of the system to the specific is the greatest,—are precisely those in which the symptoms are similar to those which the same drug would produce in a healthy person, it follows that no experiment can be valid on the subject of the dose which does not take into consideration the condition of the patient at the time, and which does not require, as preliminary conditions, first, that the subject of the experiment shall be *sick*, and second, that the symptoms of the sickness shall be similar to those which the drug with which the experiment is made would produce on the healthy subject.

In none of the experiments on which our opponents base their objections to the dose of Homœopathy have these conditions been observed.

In no case in which they have been observed has the result been adverse to that at which Hahnemann arrived.

But, in truth, these objections depend in general not so much on the result of experiments as on what is called the *a priori* improbability, and upon the seeming simplicity and triviality of the means employed in proportion to the end sought to be attained.

The improbability exists only in the minds of those who reckon respecting the living organism as they reckon respecting an inanimate machine, employing the rules of physics.

Let me quote again from Boyle: "Whereas," he says, "it is objected that so small a quantity of the matter of a specific as is able to retain its nature, when it arrives at the part it should work on, must have little or no power to relieve it; this difficulty will not stagger those who know how unsafe it is to measure the power that natural agents may have to work upon such an engine as the human body by their bulk, rather than by their subtlety and activity."

And as concerns the relative simplicity and triviality of the dose, listen to the practical Sydenham.

Speaking of the success of his new plan of treating rheumatism with whey, instead of by blood-letting, he says: "Should any one despise this method for its *simplicity*, I would have him to know that *weak minds only, scorn things for being clear and plain.* * * * The usual pomp of medicine exhibited over dying patients is like the garlands of a beast at the sacrifice."[1]

Gentlemen of the Society: In the torch-races of ancient Greece the participants ran with lighted torches, each striving to preserve the flame alive, and to hand his torch unextinguished to his successor. If the light went out in his hands he was dishonored. This was done in memory of Prometheus, who first brought fire from heaven for the benefit of men.

We have received from the generation of the pupils and successors of Hahnemann the blazing torch which the Prometheus of our system lighted at the altar of Eternal Truth. Our honor depends on the care with which we cherish it, and the state in which we, in turn, transmit it to those who shall follow us.

The especial direction which our labors should take is determined by the peculiarities of our method.

We are to increase the number of specific remedies. We are to labor diligently, as our predecessors have done, to increase our materia medica, until we shall have ascertained

[1] Russell, *History and Heroes of the Art of Medicine.*

the specific remedial properties of all substances capable of being used in treating diseases.

But more especially are we to labor to make the knowledge we thus acquire of new specifics, and the knowledge we already possess of such as we now use, more exact and definite; until we shall possess an exhaustive knowledge of each remedy, and also such a differential knowledge as shall put us in possession of all the points of resemblance and difference between each of our remedies and all the rest.

And it is in this particular province that there is the greatest present need of labor. Our materia medica is being filled with the names of drugs of which a few general properties are loosely recorded, but respecting which no exact or exhaustive knowledge has been attempted to be gained. All this must be changed if we would establish and maintain a reputation at all commensurate with the demands of modern science.

Finally, we must bring to bear upon our study of materia medica and of symptomatology all that is useful in the labors of the physiological school of medicine, in the department of the collateral medical sciences of physiology, pathology, chemistry and physical diagnosis.

For, chaotic as are the therapeutics of this school, and based on a false philosophy, we must not suffer this fact to blind us to the wonderful progress made by it in these collateral sciences, and which are as valuable to us as to it.

They supply the means of exact observation. We need, then, in part, to *re*prove our materia medica, availing ourselves of these improved means of observation; and we must employ the same in our examination of the sick.

We are called also to give ourselves to the study of *subjective symptoms*. This is our especial province, because the physiological school discards these symptoms. Prof. Böck says they are "*difficult to understand* and apt to deceive."

I have yet to learn that a study is to be avoided because it is difficult! or that a precious tool should be cast aside because it requires a skillful hand to use it.

The import of subjective symptoms, their connection with each other, their physiological and pathological significance, are all matters which it is indispensably necessary for us to elaborate and master.

And, last of all, the still open question of the dose demands our earnest study.

Conscientious, untiring labor in these departments will enable us to hold with honor our place in the great race, and to hand our torch, still blazing, to our successors.

We shall thus do our part toward making good the confident expectation of our master respecting his system.

"Our art," says he, "needs no political lever, no worldly badges of honor, in order to become something. Amid all the rank and unsightly weeds that flourish round about it, it grows gradually from a small acorn to a slender tree; already its lofty summit overtops the rank vegetation around it. Only have patience! It strikes its roots deep under ground, gains strength imperceptibly, but all the more certainly, and in due time it will grow up to a lofty God's oak, stretching its great arms, that no longer bend to the storm, far away into all the regions of the earth; and mankind, who have hitherto been tormented, will be refreshed under its beneficent shadows."

RELATION OF PATHOLOGY TO THERAPEUTICS.[1]

THE question of the Relation of Pathology to Therapeutics is one of exceeding importance, if for no other reason than this: that, by a very large portion of the medical profession, it is commonly held that the whole science of Therapeutics is *based*, in its general principles and in its special applications, upon the science and the facts of Pathology.

Before beginning the discussion of this subject, it is necessary to define the terms used in stating the question, as well as certain kindred terms.

The study of the tissues and organs of which the healthy human body is composed constitutes the science of Physiological Anatomy. It may be pursued upon the living or the dead body, provided the death resulted from violence and not from disease.

The tissues and organs of the healthy body are so fashioned as to perform, each, a special work. This act of performing its special work is called the *function of an organ*. An organ can act only during the life of the individual. Functions, therefore, can be predicated *only of living organs*. The study of the performance of their special work by the aggregation of organs which make up the body constitutes the science of Physiology.

Physiological Anatomy, then, is the science of the tissues and organs of the healthy body; *Physiology* is the science of the *functions* of the *healthy, living organism*.

When any or all of the tissues or organs of the body have suffered modifications of substance or of structure in conse-

[1] Read before the Homœopathic Medical Society of the State of New-York, May, 1863.

quence of disease, the study of these material changes, which are the results of disease, constitutes the science of *Pathological Anatomy*. This may be studied upon the living or the dead diseased organism.

When, in consequence of the action of a morbific agent, any or all of the organs of the body perform their special work in an abnormal manner; in other words, when the function of any of the organs of the body is perverted, the study of these changes and perversions of function constitutes the science of *Pathology*, which, like Physiology, can be studied only in the living organism.

Pathological Anatomy, then, is the science of the *morbid tissues and organs of the diseased body. Pathology* is the science of the *abnormal functions* of the *diseased living body.*

Diseased tissues and organs being modifications of healthy tissues and organs, it is clearly necessary to understand *Physiological Anatomy* before we can understand *Pathological Anatomy.* Just as necessary is it, for the same reason, to understand Physiology before we can master Pathology. Indeed the latter might be said to exist only by comparison with the former.

If this be true of the details of these sciences, it is no less true of their essential nature and philosophy. If one would gain a correct notion of the subject, scope, limits and relations of the science of *Pathology*, he must first have a just and exact idea of those of *Physiology.* To the latter, therefore, I propose to devote a few words.

Dr. Carpenter defines the object of *Physiology* to be the study of "the phenomena which normally present themselves during the existence of living beings," or, in another place, "the phenomena of *health* or *normal life.*"

Its object, then, is not, as it has been sometimes loosely stated, *life itself*, but the *phenomena which depend upon and result from normal life.* The science of Physiology brings these phenomena into systematic form, classifies and compares them, analyzes secondary and complex phenomena into

their simple elements, and seeks the ultimate phenomenon in which the real elementary manifestation of simple life is made, uncomplicated by secondary or related chemical or mechanical phenomena.

Virchow says, "The chief point is to obtain a recognition of the fact that the *cell* is really the ultimate morphological element in which there is any manifestation of life, and that we must not transfer the seat of real vital action to any point beyond the cell."[1]

Dr. Carpenter says, "The cell lives *for* itself and *by* itself, and is dependent upon nothing but a due supply of nutriment and a proper temperature for the continuance of its growth, and for the due performance of its functions until its term of life expires. Its chief endowment seems to be that of attracting or drawing to itself some of the various substances which are contained in the nutritive fluid in relation with it. This fluid is a mixture of a great number of components; and different sets of cells appear destined severally to appropriate these. As far as is yet known, however, the composition of the cell-wall is everywhere the *same*, being that of Proteine."

Dr. J. H. Bennett says, "Nutrition is now considered to depend upon an inherent vital property peculiar to the tissues themselves, which exercise a force at the same time attractive and selective. By its agency each tissue and gland attracts from the blood that amount of matter which is necessary to maintain it in bulk, and at the same time selects from it the peculiar substance necessary for itself and for the secretion it is destined to produce."

According, then, to the most advanced writers on this subject, we are to regard the organism as a complex which is capable of being analyzed into an aggregation of cells, of homogeneous structure, but each of which is endowed with a peculiar inherent vital power of "attraction and selection," which we recognize only in its *results* and which constitutes the only "real action of *life*" that we are capable of observing.

[1] *Cell. Path.*, Eng. ed., 3.

In the words of Virchow, "*Every animal presents itself as a sum of vital unities*, every one of which manifests all the characteristics of life."

Life, then, really consists in the exercise by each cell of its inherent peculiar attractive and selective power. Physiology concerns herself with the *results* of this exercise. A cell by virtue of its inherent power abstracts from the nutrient fluid a substance which is to serve to nourish the organ to which the cell belongs, or to furnish the secretion of that organ. If the former, then the future history of that organ, its development, its functions and its interstitial decline and dissolution and the fate of its *débris* belong to the secondary and complex phenomena to which Physiology devotes herself. With these phenomena, *life* has no exclusive connection save in the primary exertion by the cell-wall of its "inherent attractive and selective power."

Physiologists with great propriety attempt to explain by mechanical, chemical and electrical laws the secondary phenomena of the organism, the relations of different tissues and secretions to each other and their mutual reactions. But they make no attempt to explain the action of the cell-wall which is the *ultimate and essential phenomenon of life*. They accept this as an ultimate fact, they recognize a power peculiar to the wall of each variety of cell and different from that of any other cell, and they deal with the *results of the exercise of this power*.

Physiology having been defined as the science of the phenomena of *normal life*, Pathology has been declared by the same writer (Dr. Carpenter), to be the science of the phenomena of "*disease* or *abnormal life*."

If the mutual reaction of two secretions be different in any individual case from that which is observed in the healthy organism, one or other of the secretions must be of an abnormal character. This must have resulted from an abnormal exercise by the cell-wall of that inherent selective power, by

virtue of which the secretion was made. The essential and ultimate act of *disease* is found, like that of *life*, to consist in some abnormal exercise by the cell-wall of its inherent vital power. As we recognize the existence of this power in the healthy cell only through its action and its result, viz., the function of secretion and the substance secreted, so we recognize disease, which is an abnormal exercise of this power, only through its action, viz., the perverted function, and through its result, viz., an abnormal secretion.

"Not unfrequently," says Dr. Bennett, "this attractive and selective power in the tissues is deranged, producing increase or diminution in growth or secretion, general or partial. Not unfrequently the selective power appears to be lost, and the attractive power much increased," etc., etc. From this alteration of these forces, diseased conditions of these fluids and tissues result. The secondary and complex phenomena resulting from the reaction of abnormal tissues are analyzed by Pathology and referred to this elementary deviation in the selective and attractive forces of the cell-wall. But the nature and intimate cause of this alteration, which is, however, the essence of disease, Pathology does not attempt to explain or apprehend, knowing it only by its effects.

As, therefore, Physiology concerns herself with the *results of life*, so does Pathology take cognizance of the *results of disease*. For it must be repeated that as the functions of healthy organs and the tissues of the healthy body which are respectively the subjects of Anatomy and Physiology are not *life*, but only results of life, so the abnormal functions and the altered tissues of the diseased body, which are respectively the subjects of the science of Pathology and Pathological Anatomy are not *disease* itself, but only the results of disease.

These distinctions, it will be perceived, have a direct and most important bearing upon the science of Therapeutics, which has for its object the cure of disease.

The process by which the secretion of any fluid of the body is accomplished cannot be explained. It is an exertion of that peculiar vital power of which we have spoken as inherent in the cell-wall. The secretion of the gastric juice and of the bile, for example, is wholly inexplicable. But when, after being secreted, these or similar fluids are brought into contact with each other or with any foreign substance, whether within or without the body, the actions and reactions which ensue are explicable by chemical, mechanical or electrical laws, and these explanations constitute the object of Physiology. It is obvious that these phenomena may be of great complexity and that the explanations may require to state the conditions of several successive series of reactions, which intervene between the external fact or symptom as viewed by the observer, and that ultimate action of the cell-wall which Virchow declares to be "the only real vital action of the living organism"—or, on the other hand, the phenomena may be simple and the explanation may be complete by a simple and direct reference to the selective action of the cell-wall. Thus, for example, the fact of salivation, as noticed by an external observer, is made up of the discharge of the salivary secretion into the mouth. The explanation refers us at once to that selective power of the cell-wall of the salivary glands which extracts from the nutritive fluids the constituents of this peculiar secretion which, as such, is not found anywhere in the body except in the salivary ducts as the *result* of the process we have just described.

On the other hand, the fact of *urination* is made up, so far as the external observer is concerned, of the discharge of urine from the urethra and the emptying of the urinary bladder. The physiological investigation, however, shows the process to be quite complicated, and the explanation carries us through several series of phenomena, before we arrive at the ultimate fact. Thus, we trace the urine through its reservoir, the bladder, up the ureters to the tubuli of the

kidneys, where it has probably received, if not the whole, at least a great portion of its watery constituents; and this contribution may require a further explanation by the mechanical laws of endosmosis and exosmosis whereby this constituent was separated from the blood. Not tracing this constituent further, for the present, by asking, as we should logically be required to do, how this substance came to be contained in definite quantity in the blood, we seek the other constituents of the urine and find the urea and possibly some other of them eliminated by the Malpighian bodies from the blood through the agency of the cell-wall. And yet even this can hardly be called the ultimate vital act, which will require to be sought by a still deeper analysis. For although the cells of the Malpighian bodies do separate the urea from the blood, yet they do not manufacture it from the constituents of that fluid. They find it there already formed, and an exhaustive analysis of the physiological fact of urination would require us to seek the origin of the urea in the blood. This would lead us to the study of the nutrition and decay of the nitrogenized tissues, and in the cell-walls of *these* we should first meet with that ultimate vital act, in which alone, of all the processes which urination involves, *life*, pure, simple and inexplicable, is concerned.

Physiology goes somewhat further than this analysis of the actual normal functions of the living organism. Under the name of Experimental Physiology she not only seeks to apply the test of synthesis to her analysis, but she endeavors to solve her problems by the substitution of analogous substances for some elements of the nutrient fluid, or for some of the secretions of the organism, and thus light is thrown not merely on the processes of the living organism, but also contributions are made to the derived science of *Pathology*.

Let us now take a similar view of the science of Pathology, which deals with abnormal functions as Physiology does with

normal functions, and which might almost, by a license of speech, be called the science of *Morbid Physiology*.

The fact of morbid salivation is, to the observer of objective occurrences, only a discharge of abnormal salivary fluid. The pathological explanation, like the physiological, leads us to the selection from the blood by the cell-wall of the glands of the substances out of which it fabricates the salivary fluid. If the blood be normal and yet the saliva be, as we have supposed, abnormal, then the mysterious power of the cell-wall must have been perverted, altered or impaired. This manifestation of life must have become a manifestation of *abnormal life*—which is disease. *Here*, then, is the *seat of disease*—just where we found the seat of *life*—in the inherent attractive and selective power of the cell-wall. This, then, is the *nature of disease*, a modification of that unknown and inscrutable power, *life*.

"*Health*," says Carpenter, "or *normal life;*" "*disease or abnormal life*."

"Not unfrequently," says Dr. Bennett, "this attractive and selective power in the tissues is deranged. Not unfrequently the selective power appears to be lost and the attractive power greatly increased, and thus," after a few intermediate steps described, "the phenomena which we call *inflammation* ensue."

To take, now, an example of a more complex series of phenomena, the fact of the evacuation of morbid urine, simple as it may appear to the observer of external phenomena only, yet leads us, in our search after the original act of "abnormal life" in which the morbid excretion had its origin, to examine first, whether the change was purely a chemical change, originated and effected while the urine was retained in its reservoir, the bladder—for if so, it differs little from a corresponding change which occurs daily in the "*vase de nuit*"—secondly, whether the urine derived its morbid character from the admixture of morbid secretion, or effusion from the surface of the bladder, of the ureters,

of the pelvis of the kidney, or from the tubuli; from each of which it might receive blood, or mucus, or pus, or, perhaps, liquor sanguinis alone holding fibrine in solution, or finally solid fibrinous casts; or, thirdly, whether the morbid change in the urine came from a loss, on the part of the cell-walls in the Malpighian bodies, of their inherent vital selective power, whereby they are enabled to separate from the blood those substances which it is their province in the healthy body to separate; or, finally, whether the blood no longer contains, as in a normal state it should do, those elements which it is the function of the kidney cells to eliminate from it,—in other words, whether the whole train of processes which eventuates at last in the evacuation of abnormal urine, may be traceable to an abnormal interstitial nutrition and destruction of the nitrogenized tissues; all these are questions which it is the legitimate and very useful business of Pathology to solve.

Now, be it observed, whether the cause be traced to abnormal action in the cell of bladder, ureter, kidney, or in that of the muscular or cerebral tissue, we arrive always at the fact that as the action of the cell-wall in the healthy body constitutes the only real *vital* action of the organism, the only unmixed manifestation of life; so the modified and abnormal action of the cell-wall in the diseased body constitutes the only *real* action of abnormal vitality in the organism—the only unmixed manifestation of disease.

It may be objected that no account has been taken in what we have said, of the action of the nervous system, to which, nevertheless, both in the case of morbid salivation and in that of morbid urine, the alteration in the salivary gland and in the kidney respectively may have been attributable. But this objection simply adds another step to the analysis. For every action of any part of the nervous system involves an expenditure of a portion of nervous substance, that is, a process of interstitial destruction and reconstruction, and this process is effected only through the

attractive and selective power of the cell-wall of the nervous tissue. Thus are we brought again face to face with this inevitable mysterious power, the abnormal action of which constitutes *Disease.*

We are now in a position to discuss the relations of this science of Pathology, which treats of the phenomena of the diseased organism, to the science of *Therapeutics*, which proposes to *cure disease.*

It has been stated that a large portion of the medical profession hold that the science and practice of Therapeutics are based upon the science and facts of Pathology. It is held that the explanation of the phenomena of the morbid organism, which Pathology furnishes, gives the key to the procedures which should be adopted for the restoration of the morbid organism to a healthy condition. If, for example, it be ascertained through the investigations which Pathology makes for us, that a certain series of morbid symptoms may be traced to an abnormal condition of the gastric fluid,—a too great acidity, for example,—"it is," says Dr. Bennett, "the discovery and removal of such causes as this that constitute the chief indications of the scientific practitioner." Let us see in what way Dr. Bennett proposes that this "removal" shall be accomplished. He says, supposing another case: "The capillary vessels become over-distended with blood, and the exudation of the liquor sanguinis to an unusual extent takes place, constituting inflammation. How is this to be treated? In the early stage, topical bleeding, if directly applied to the part, may diminish the congestion, and the application of cold will check the amount of exudation. But the exudation, having once coagulated outside of the vessels, acts as a foreign body, and the treatment must be directed to furthering the transformations which take place in it, and facilitating the absorption and excretion of effete matter. This is accomplished by the local application of heat and moisture, the internal use of neutral salts to dissolve the increase of fibrine in the blood, and the employ-

ment of diuretics and purgatives to assist its excretion by urine or stool."

In this plan of treatment, we have a proposition to adopt mechanical measures—blood-letting—for the diminution of a congestion; chemical means—neutral salts—for the solution of the exudation; and mechanical means again—diuretics—for the removal of the dissolved exudation from the blood.

But it cannot fail to attract the attention of every reader that this plan proposes to deal only with the *results of disease*—not with the disease itself—for the congestion is a primary result of the abnormal action of cell-wall, and the exudation which results from the congestion is a secondary result of the same abnormal action of the cell-wall. This is clearly shown by Dr. Bennett's words already quoted, but which we again cite: "Not unfrequently the selective power of the cell appears to be lost, and the attractive power so much increased, that the liquor sanguinis is drawn out through the vessels, so that its fibrine coagulates in a mass outside of them. This result, preceded or accompanied by certain changes in the vessels themselves, and more or less stagnation of the current of the blood, constitutes the phenomenon hitherto described as inflammation."

It is here most distinctly stated, and most conclusively shown, that the cause of the phenomena of inflammation lies behind these phenomena of congestion and exudation, and is to be found in a modification of that mysterious vital power of the cell-wall. It follows from this that a treatment which is addressed to these resultant phenomena, and not to their cause, must be but a palliative treatment. It can in no sense be called a radical or a rational treatment. To remove or assist in removing from the body or any part of it the results of morbid causes may alleviate suffering, may enable the patient to hold out longer under disease, just as pumping out a leaking ship may postpone the time when she will sink, may increase her chances of getting into port before

she sinks,—may thus in fact be the means of saving her. But as, certainly, pumping is only a palliative mode of treating a leaking ship—as the stoppage of the leak is the only radical treatment imaginable—so in the case of inflammation the only radical treatment would be one which addressed itself directly to the cell-wall, and in such a way as to restore to it the just amount and proportion of its now perverted "inherent attractive and selective power."

It is evident that when this had been accomplished, congestion would cease to be produced, and in consequence of this cessation there would be no further exudation. It might now be within the province of Pathology to indicate some mechanical or chemical way of aiding or hastening the removal of the results of disease—the congestion and exudation—but this would be only a sequel of the *curative treatment.*

A *curative treatment*, then, must address itself directly to the cell-wall, which is the ultimate seat of disease. And every method which is directed to the *results of disease*, which are the proper subjects of Pathology and Pathological Anatomy, must, of necessity, be a *palliative* method.

But, it may be objected, we are unable to plan any such *curative* treatment, for the reason that we do not understand the nature of this cell force, nor the nature of its perversion, in any case. To recur to our simile of the ship: it is sometimes impossible to find the leak, or, if its location be known, to stop it. Nothing remains but *palliation*. This statement, as applied to disease, is correct in only a few instances, the number of which is becoming continually diminished by the progress of science. It is true that we do not understand the nature of the cell force, whether normal or perverted. Neither do we understand the nature of chemical affinity, nor the *modus operandi* of chemical reaction. Yet we have an experimental knowledge of each, and we know the peculiar chemical affinities and reactions of a vast number of bodies.

In like manner we may learn the reaction of foreign bodies introduced into the circulation upon the inherent forces of the cell-wall. Reasoning upon the general physical law that "like effects are produced by similar causes," we conclude that, if in any case, having introduced into the circulation of the healthy subject a foreign body, we observe the phenomena of congestion and exudation, with stagnation of the current of the blood, identical with those known to us as the phenomena of inflammation, then the peculiar inherent powers of the cell-wall must have been affected by this foreign body in precisely the same way (whatever it be) as that in which any accidental exciting cause of inflammation affects these forces. We thus discover re-agents which act *directly* upon the inherent power of the cell-wall, and we thus, by experiments upon the healthy, determine precisely the limits and direction of their action. This knowledge is rendered possible and exact by the science of Pathology, which enables us to trace out and analyze the *results* of intentionally induced abnormal cell-action, just as it does those of *accidental disease.*

This is not a matter of mere speculation. It is well known to every physician that almost every drug, every substance in fact which is not a simple aliment, has, besides any chemical affinities it may possess for some of the secretions of the living organs, a certain peculiar power of modifying the functions of some of the organs of the body, a power which is inexplicable and is recognized only by its results. Examples of this power of drugs are found in that of Tartar emetic to produce changes which result in congestion and exudation, of Opium to produce changes which result in narcotism, etc. These, which are known as the specific properties of drugs, are evidently the properties by which they modify the forces of the cell-wall, and from which all observed Pathological (or Pathogenetic) and Pathologico-anatomical phenomena result.

And it is manifest that the specific modifying power of

each drug can be surely ascertained, as an absolute property, by testing it upon the healthy living organism. And it follows as clearly, from all that has been said, that the application of specific drugs which have a direct action upon the cell-wall whose inherent forces are acting abnormally under the influence of disease, is the only direct and radical method of curing disease.

The task of finding out by what formula the specific action of drugs upon the cell-forces of different tissues should be brought to bear upon the perverted cell-forces in any given case of disease, comes under the science of Therapeutics and lies beyond our present province. It is the problem of the Therapeutic Law.

From all that has been said, the Relations of Pathology and Therapeutics may be deduced in a few words.

Physiology and Pathology themselves teach us that the science of Pathology can in no sense serve as a basis or foundation of the science of Therapeutics. They show us that whereas Pathology is the science of disease based on a theory of observed morbid processes, Therapeutics, when truly regarded, is a science of cure, based on a theory of cure and resting on a foundation of experiment. Although not the basis of Therapeutics, Pathology must yet be a most important instrument in the practical application of the science of Therapeutics.

The problem in the art of medicine being to apply to the abnormally acting cell-wall that remedial agent which is capable of acting directly upon it, and in such way as to restore its normal action, it is clearly indispensable that the physician be able to trace the morbific agent through all its complex resultant phenomena up to the original perversion of cell-force, from which the whole disorder springs; that he be able to analyze secondary and tertiary series of phenomena into their simple elements. And in this work Pathology is the instrument of which he avails himself.

I cannot refrain, in conclusion, from rendering homage to

that wonderful prevision of genius by which, in an age when Pathology, as we understand it, was unknown, Samuel Hahnemann anticipated all that we have said, and all that the most advanced writers of our day have taught, respecting the scope and influence of Pathology in relation to Therapeutics.

The symptoms of the urinary organs in connection with the discharge of morbid urine would at one time have been regarded as the proper subject of treatment. But Pathology has now taught us to trace these symptoms back to the kidney, and beyond the kidney to the blood, and beyond the blood to the nutrition and the destruction of all the nitrogenized tissues. As Dr. Carpenter remarks, "When, for example, the urine presents a particular sediment, our inquiries are directed not so much to the kidney itself, as to the constitutional state which causes an undue amount of the substance in question to be carried off by the urinary excretion, or which prevents it from being (as usual) dissolved in the fluid." To confine the attention, therefore, in prescribing for a given case, to the immediate organ the perversion of whose functions is most obviously pointed out by the prominent symptoms, is to disregard the clearest indications of Pathology. We must analyze these obvious symptoms and must include their remotest elements in our indications. Nay, these remotest elements—the constitutional disturbances, for instance, of which Carpenter speaks—are even more important indications for treatment than the more obvious and objective symptoms. But how can we analyze these more obvious symptoms, and ascertain those "constitutional disturbances" in which they have their origin? In no other way than by a study of the functions of the entire organism—in what way and to what extent they are performed in an abnormal manner. But this brings us at once to that rule on which Hahnemann so strongly insisted, that the entire organism of the patient should be examined in every possible way, and that the "totality of the symp-

toms" should be made the basis of the prescription; nay, that the constitutional, general symptoms are often more conclusive as to the proper treatment than the more obvious local symptom. The grand old master reached at a single bound the same conclusions to which the labors of a half century of able Pathologists have at length, with infinite research, brought the medical profession.

And those of our school who insist upon Pathology as a *basis* of Therapeutics, who look upon the single objective symptom and its nearest organic origin as the subject for treatment, and who deride the notion of prescribing upon the totality of the symptoms, and claim to be more than mere symptom-coverers, in that they discover and aim to remove the *cause* of the disease—these colleagues are as false in their Pathology, according to the highest Old-School authority, as they are faithless to the doctrines and impotent as to the successes of the founder of the Homœopathic School.

PRIMARY AND SECONDARY SYMPTOMS OF DRUGS AS GUIDES IN DETERMINING THE DOSE.

As preliminary to an intelligible discussion of this question, we must briefly define primary and secondary symptoms respectively, and state how, in our judgment, the discrimination between them bears upon the selection of the remedy. And this, notwithstanding these questions have been elaborately and lucidly discussed by other members of this bureau. For it will not have escaped the reader's observation that these terms are used with different significations by different writers

Symptoms may be called primary as being first in order of occurrence, in comparison with others which, occurring at a later period, are, with reference to time, secondary to them.

Or, symptoms may be called primary as being, in a sense, the exciting cause of other and opposing symptoms, which are then secondary to, as being contingent upon, the former.

Or, symptoms may be styled primary as being of greater importance or significance than others called, therefore, secondary.

The distinction, then, may be based on considerations of time, of opposition in nature, or of rank. And it is important not to confound or combine these ideas in our discussions.

Since most of our traditional notions on this subject originated in Hahnemann's utterances upon it throughout his writings, I will briefly repeat his views before stating my practical conclusions

In an essay entitled *Suggestions for Ascertaining the Curative Powers of Drugs*, published 1796 (S. W., 312),

Hahnemann says: "Most medicines have more than one action; the first a *direct* action, which gradually changes into the second (which I call the indirect secondary action). The latter is generally a state exactly the opposite of the former. In this way most vegetables act. But few medicines are exceptions to this rule, *e. g.*, metals and minerals." He illustrates what he means by the secondary action in the following note: "Under Opium, for example, a fearless elevation of spirit, a sensation of strength and high courage, an imaginative gayety, etc., are part of the direct primary action of a moderate dose; but after eight or twelve hours an opposite state sets in—the indirect secondary action; there ensue relaxation, dejection, diffidence, fear, loss of memory, etc."

In the preface of the *Fragmenta de Vir. Med. Pos., etc.*, 1805, Hahnemann says: "Simple drugs produce in the healthy body symptoms peculiar to themselves, but not all at once, nor in one and the same series, nor all in each experimenter; but to-day perhaps these, to-morrow those; this first one in Caius, the third in Titus, but so that on some other occasion Titus may experience what Caius felt yesterday.

"A certain drug evokes some symptoms earlier and others later, which are somewhat opposed and dissimilar to each other; indeed *may* be diametrically opposed. I call the *former* primary, or of the first order, and the latter secondary, or of the second order.

"For each individual drug has a peculiar and definite period of action in the human body, longer or shorter, and when this has passed, all the symptoms produced by the drug cease together.

"Of the drugs, therefore, the effects of which pass over in a brief space of time, the *primary* symptoms appear and disappear within a few hours. After these the secondary appear and as quickly disappear. But the exact hour in which any symptom may be wont to show itself cannot be

positively determined, partly because of the diverse nature of men, partly because of different doses. * * *

"I have observed some drugs the course of whose effects consisted in two, three or more paroxysms, comprising both kinds of symptoms, both the *primary* and the *secondary;* the former, indeed, as I have stated in general terms, appeared *first* and the latter *second.* And, sometimes, it seems to me I have seen symptoms of a kind of *third* order.

* * * * * *

"Under the action of moderate or small doses, the symptoms of the first order come chiefly to view; less frequently those of the second order. I have chiefly preserved the former, as most suitable to the Medical Art and most worthy to be known."

Finally he speaks of a class of symptoms which he denominates "reliquias," generally the effects of very large doses, and which seem to indicate or depend upon more or less permanent alterations of tissue, including the symptoms of the "agony" in fatal cases.

As illustrating these views, I quote remarks prefixed or appended to the symptomatology of several of the drugs mentioned in the *Fragmenta.*

In a note to Aconite, Hahnemann says: "Through the whole course of action of this plant, its effects of the first and second order were repeated in short paroxysms, two, three, or four times before the whole effect ceased (eight to sixteen hours)." And he describes these effects as follows:

"Coldness of the whole body and dry internal heat. Chilliness. Sense of heat first in the hands, then in the whole body, especially in the thorax, without sensible external heat.

"Alternating paroxysms (during the third, fourth, and fifth hours); general sense of heat, with red cheeks and headache, worse on moving the eyeballs upward and laterrally, then shivering of the whole body with red cheeks and hot head; then shivering and lachrymation with pressing headache and red cheeks."

In a note to Chamomilla, Hahnemann says: "The course of its action is run in paroxysms of several hours' duration, comprising symptoms of each order, free spaces or remissions being interjected, so, nevertheless, that in the earlier paroxysms, the symptoms of the first order, in the later, those of the second order predominate."

In a note to Ignatia, he says: "Ignatia is wont to display the curriculum of its operation in several paroxysms comprising both orders of symptoms, and repeated at intervals of several hours;" and concerning the mental symptoms: "Inconstancy, impatience, vacillation, quarrelsomeness, wonderful mutability of disposition, now prone to laughter, now to tears," he says: "These mental symptoms are wont to be repeated at intervals of three or four hours."

Hahnemann's teachings on this subject in the *Organon* (in which all the editions substantially agree) have been so fully given by Dr. C. Wesselhœft in the preceding section of the report of this bureau, that I am spared the necessity of quoting them.

It appears that Hahnemann, in the *Fragmenta* and the *Organon*, teaches that among the symptoms of a drug, there appear series which are opposed to each other in different degrees of diversity, from being "somewhat opposed" to "diametrically opposite;" and that, of these series, that which occurs *first* in order of time, is to rank among the *primary*, and that occurring subsequently among the *secondary* symptoms. But he calls attention to the fact that there are some kinds of symptoms in every proving, to which there can be no series of an opposite nature, *i. e.*, to which an *opposite* cannot be predicated; for, he says, "Our organism always bestirs itself to set up in opposition to this effect [first drug action], the OPPOSITE condition, WHERE SUCH A CONDITION CAN EXIST."[1]

In his definition of primary and secondary symptoms, therefore, Hahnemann blended the elements of time and of

[1] *Organon*, 2d and 3d ed., § 74; 4th ed., § 63.

causation or nature (viz., that these classes were opposed in their nature). The secondary symptoms were not an independent series, but were secondary by virtue of their relation of opposition in nature to a series of preceding symptoms. And such symptoms as did not in their nature admit of an opposite condition (as, for example, pain, cutaneous eruption, etc.) could not be called *primary*, because, in the nature of things, they could not be followed by an opposite class of symptoms. Nor could they be called *secondary*, because, in the nature of things, they could not have been preceded by an opposite series, which could stand to them in the relation of *primary* symptoms. Hahnemann, then, appears to have recognized in the pathogeneses of drugs, symptoms which being opposed in nature could be arranged into series of *primary* and *secondary*, and other symptoms not susceptible of such arrangement.

He distinctly tell us[1] that the *primary* or positive symptoms of drugs are those on which we are to base our prescriptions.

These statements in the *Organon*, as quoted by Dr. Wesselhœft, embrace not only a description of various classes of symptoms as observed by Hahnemann in drug-proving, but also a theory of the nature and genesis of these various classes. A man's observations of natural phenomena, if he be a keen and accurate observer, as Hahnemann unquestionably was, are generally correct. His theoretical explanation of them is pretty sure to be tinctured with the philosophy of the period in which he wrote, and is not likely to be accepted without qualification by men of a subsequent period. And, at the present day, few would accept Hahnemann's explanation of the genesis of primary and secondary symptoms as representing respectively a state of passivity followed by a state of intensified activity on the part of the vital force; this conception of a vital force, in the sense in which Hahnemann used the term, being one which, itself, has been

[1] *Organon*, 2d and 3d ed., § 59; 4th ed., § 152.

discarded by most physiologists. But the rejection of the explanation offered by Hahnemann does not involve the rejection of the observations to which he attached it.

Among the symptoms which he called *primary* (Erst wirkungen), Hahnemann recognized the occasional occurrence of what he called alternate (Wechselwirkungen), opposed, sometimes contradictory symptoms, which, nevertheless, were not *secondary*. He does not tell us how to recognize these, nor how to distinguish them from the secondary symptoms. But he does give us instances of what he regards as secondary symptoms, as follows:[1] "The gayety which follows the use of coffee is a *primary* symptom; the subsequent drowsiness and lassitude are *secondary* symptoms. The sleep which follows Opium is a *primary* and the subsequent insomnia a *secondary* symptom; the purging of cathartics is a *primary* and the subsequent constipation a *secondary* symptom. The constipation of Opium is a *primary* and the subsequent diarrhœa a *secondary* symptom." And consistently with Hahnemann's instructions, we should not expect to base our prescriptions on these secondary symptoms.

But when we examine Hahnemann's remarks on the individual drugs of the *Materia Medica Pura*, we find deviations from his definitions and illustrations as given in the *Organon*. In the preface to Belladonna we read:[2] "There is no known drug of long action which expresses itself in so manifold (two- and three fold) alternate conditions as Belladonna. Only compare symptom 15 with 16 and this with 17, 56 with 58, and this with 60, 61 and 114, and these with 113 and 152; again, 62 with 63, 64, and these with 70, and 62 with 72, 158 with 159, and this with 160 and 165, and this with 163; and 172 with 174 and 175, and these with 176. Of *none* of these alternate conditions (Wechselwirkungen) can it be said that they are beyond the primary action."

[1] *Organon*, 2d and 3d eds., § 76; 4th ed., § 65.

[2] *Materia Medica Pura*, 1st ed.

The symptoms thus referred to describe opposite conditions as follows:

Contracted pupils and dilated pupils.

Abdominal pains compelling to *bend backward* and to *sit still*,—to move forward and not admitting of motion.

Suppressed stool and urine and involuntary stool and micturition, and constant tenesmus.

Sleeplessness and deep slumber.

Raging delirium and wild fear, and foolish madness, etc.

It is evidently Hahnemann's meaning, and surely experience justifies him, that Belladonna may be given (other symptoms corresponding) when either the one or the other (the opposite) of these conditions is present. And these opposites belong to the class described in the *Organon* as "*secondary*, and on which we are told, we are *not* to base our prescriptions.

In the introduction to Nux vomica (*Materia Medica Pura*, 1st ed.) Hahnemann says: "The symptoms of a single dose of Nux vomica are wont to recur several days in succession, at the same time of day, even at the same hour, or every other day. Hence the usefulness of this drug in some typical diseases when the symptoms otherwise correspond. Besides this periodicity of the symptoms, and besides the alternation of heat and cold, there follow also upon one another, here and there (as is the case also with other drugs), symptoms which differ very much from one another, and *appear to be opposed to each other*, although they all belong to the primary action of the drug. We may call these *alternate actions* (Wechselwirkungen)." And among the symptoms of Nux vomica, Hahnemann calls attention to 232 and 233 "*anorexia*," as contrasted with 236–238, "great appetite," and says these are alternate actions, and belong to the primary symptoms, and are, therefore, to be used as bases of prescriptions. He refers also to 369–374, "Constipation with tenesmus," etc.; and to 357–359, "Diarrhœa with desire and tenesmus," etc.; and says, in a note, "Diarrhœa,

constant, abundant, strictly so called, is not, according to my observation, to be expected in the primary action of Nux vomica; and that which here appears among the symptoms as diarrhœa is partly very small, mostly mucous discharges with tenesmus and pain," etc. Again, in a note to 456, he says: "Discharge of mucus from the nose is an alternate action with dry obstructions of the nose." There is, clearly, a discrepancy between Hahnemann's general propositions in the *Organon* and his practical instructions in the *Materia Medica Pura*. For we find from the latter that Belladonna, for instance, may be given for the *primary* symptom, "sopor," as well as for what in the *Organon* is called the *secondary*, but in the *Materia Medica Pura* the *alternate* symptom "sleeplessness," and that Nux vomica may be given for the *primary* symptom "constipation," and likewise for what is called in the *Organon* the *secondary*, but in the *Materia Medica Pura* the *alternate* symptom "diarrhœa"

The very terms *primary* and *secondary*, as thus illustrated, seem to imply a succession of symptoms, more or less opposed in character, and all of them differing from the equilibrium of function which we recognize as health. The instances given, and, indeed, the only possible instances, relate to functions of which a "more" or "less," or an "opposite," may be predicated; as, for example, temperature, sleep, certain mental conditions, and the secretions and excretions generally. Thus we may have an unnaturally prolonged sleep or wakefulness, gayety, or despondency, and a *plus* or *minus* of sweat, alvine discharge, urine, etc., etc. But how could we have an opposite condition to any specified pain or subjective sensation, to parenchymatous deposit, to cutaneous eruption, etc., etc.? The absence of these phenomena would be *pro tanto* a state of health; it would not be an opposed morbid condition or sensation.

The possibility, then, of classifying symptoms into primary and secondary on the basis of the relative nature of the symptoms, is not co-extensive with symptomatology; it is partial,

confined to a moderate number of conceivable morbid phenomena.

Shall we, then, in the second place, base the distinction on the element of time, and call the symptoms which first occur *primary*, and those which come later, *secondary?* Where then shall we draw the line? how many hours or days shall we allow for the development of primary symptoms? In view of the immense differences in the rapidity with which the curriculum of action of different drugs is run, it is obvious that a special rule must be established for each drug. Nor is this the only difficulty. The results of different doses on the same provers, and of different doses or even of the same dose on different provers, are so various that, 1st, as Hahnemann intimates in the preface to the *Fragmenta*, the symptom which appears in one prover to-day will not appear for several days in another prover; and, 2d, a very small dose may produce only one series of symptoms; a larger dose two series of opposed symptoms; a still larger dose two series differently opposed; and a very large dose again only one series. This point has been so well illustrated by Prof. Allen, with whose views I am glad to express my entire concurrence, that I need not dwell upon it, but may content myself with two illustrations from our Materia Medica.

On looking over the register of symptoms of Argentum nitricum,[1] we find reported as occurring early in the proving, irritation of the bladder and urethra and increased frequency and quantity of urine, and as occurring later in the proving, diminution in frequency of micturition and in the quantity of urine. Surely one might pronounce the former to be *primary* and the latter *secondary* symptoms. But on examining the provers' *day-books* we find that the majority of the provers (being those who took large doses) report the *former* and *not the latter symptoms.* It was the prover who took the 30th, who reported diminished urine, and *he* did not report any increase at any time.

[1] *Oest. Zeit.* I.

These symptoms, therefore, which appear in the register to be opposed, and properly distinguishable as primary and secondary, did not bear to each other any relation of apposition or correlation, as they might have done had they occurred in the same individual. They are different, unrelated, independent effects of different doses in different individuals. And let me suggest, in passing, that the beautiful pictures of primary and secondary effects of drugs which we find in works of Old-School writers, and which have been made the basis of "laws of the dose" by writers of our own school, are composite pictures made up from a variety of observations on patients and from cases of poisoning, and bear no more resemblance to a pathogenesis on a single individual, than the composition of an artist which has the mountains of Ecuador covered with the forests of Oregon and decked with the flowers of Java presents to a faithful landscape from nature.

Most of the provers of Tellurium taking the 3d trit. had, on the first and subsequent days, symptoms of the general sensibility, of sweat, of the skin, of the bladder, etc., etc. But one prover who took the 4th trit. had no symptoms at all until the fourteenth or fifteenth day, when cutaneous symptoms affecting the ear appeared and were very persistent and troublesome. Were these symptoms secondary because they came later than *other* symptoms in *other* provers? And secondary to what? How can John's lumbago be secondary to James's toothache? But during the second month this same prover, his ear symptoms having vanished, had symptoms referred to the dorsal spine. Were these secondary to the ear symptoms because they came later? Certainly as regards *time* they were secondary, because later. But being in nature wholly unrelated, neither opposite nor similar, they cannot be called secondary as regards nature nor as regards rank or value. Both have been repeatedly verified in practice.

Again, we are told[1] that coldness, a condition correspond-

[1] "The Dose," by E. M. Hale, M.D., *N. A. J.*, ix. 265.

ing to the chilly stage of fevers, is the *primary* effect of Aconite, and that a state corresponding to the hot stage of fevers is the *secondary* effect of that drug. Let us hear Hahnemann. In the introduction to Aconite[1] he says: "Aconite is one of a few drugs whose *primary action consists in several alternating conditions of chill or coldness and heat.*" And now let us study the day-books of the Austrian provers of Aconite.

Rothausl took tincture of Aconite in doses regularly increasing from six drops daily to fifteen drops daily for nine days, when, feeling powerful effects, he ceased taking it and noted his symptoms.

From the second to the eighth day inclusive, he had the following constantly recurring symptoms: restlessness at night; bad dreams; *unnatural heat of body;* rawness and increased secretion in the larynx; cough; vertigo; headache. On the ninth day, after midnight, severe chill in paroxysms of shivering, starting from the præcordia, lasting two hours, followed by burning, dry heat, with frequent, feverish pulse; and this followed by moderate sweat.

For the next six days he had various troublesome symptoms affecting the chest and limbs, and on the sixteenth day of the proving he had again, at night, a febrile paroxysm consisting of chill, heat and sweat, the first less severe, the last more abundant than on the ninth day.

On the seventeenth day, at night, a similar febrile paroxysm. Then for seven days symptoms of increasing severity in the head and chest, ending with hæmoptysis on the nineteenth day, and finally, on the twenty-fourth day, a very severe and well marked and defined neuralgic head and face ache.

How can the ingenuity of the most ambitious lawgiver find a pretext for dividing the symptoms (especially the febrile symptoms) of this excellent proving into primary and

[1] *Mat. Med. Pura*, 1st ed.

secondary? A febrile paroxysm occurred on the ninth, the sixteenth and the seventeenth days; before it and after it were well-marked symptoms of the chest and extremities. Which shall be primary and which secondary if date of occurrence determine the question? Which, if nature or if rank determine it?

Certainly, if lateness of occurrence stamp a symptom as *secondary*, then the neuralgic head and face ache, the very last symptom reported by Rothausl, must be classed as *secondary*. Not so fast, however! In the proving of Aconite by Zlatarovich with the second decimal, which he took in increasing and very large doses for seven days without effect, the very first symptom was a violent neuralgic head and face ache, almost identical with that described by Rothausl on his twenty-fourth day.

It appears, then, that Rothausl's *last* symptom was Zlatarovich's *first*. If the time of occurrence determines the class, we must rank Rothausl's headache among the secondary, and Zlatarovich's identical headache among the primary, and thus we have the same symptom in each class, which is a *reductio ad absurdum*.

It will be noticed that these identical symptoms, produced at different times in different provers of Aconite, were produced by different doses. The opposite symptoms of Argentum nitricum in different provers resulted from different doses.

Prof. T. F. Allen has shown how greatly the results of different doses vary. Dr. Sharp shows that, in the same prover, Aconite has four different kinds of action on the heart, as shown by the radial pulse, depending on the dose, and that in only two of these is one series of symptoms followed by an opposite series.

Hahnemann, who, it must be remembered, had an immense experience as a drug-prover, and who brought to the work a devotion and powers of observation and analysis rarely equaled, was well aware of the fact that the appearance of apparently contradictory symptoms in a proving is

greatly dependent on the *dose*, as appears from § 66 of the *Organon* (4th ed.), in which, for this reason, he recommends the use of small doses in proving.

Passing for a moment to the second division of the subject, the value of primary and secondary symptoms as guides in selecting the remedy, there are many drugs which, having certain constant characteristic symptoms, have also series of alternating symptoms relating chiefly to the secretions. Among them we may mention *Veratrum album*, which has (122–124) "Thin stool passing unnoticed with flatus. Frequent liquid stools. Liquid stools unnoticed with flatus. Diarrhœa of acrid fæces, etc." And also (127–139), "Constipation from thickness and hardness of fæces. A desire and compulsion to stool in the upper abdomen, and yet no stool, or a very difficult one, as if from inactivity of the rectum, and as if the rectum took no part in the peristaltic motion of the upper intestines." Also Hahnemann quotes from Greding, "Diarrhœa with copious sweat" and "long-continued constipation"

The efficacy of Veratrum in the treatment of diarrhœa of an appropriate character is universally conceded in our school. And in my own practice, Veratrum has for many years been a frequently used and highly valued remedy for *constipation* in persons of all ages, but especially in infants and young children, in whom digestion appearing to be well performed, the evacuation of fæces appears nevertheless to be impossible because of the inertia of the rectum—a fact demonstrated by the circumstance that a healthy stool can be procured almost at will by irritation of the rectum, as by the common practice of introducing into the anus a piece of soap or an oiled paper, or a rubber bougie. We have here the apparent anomaly of the same remedy equally efficacious in diarrhœa and constipation.

Nux vomica furnishes a similar example. Its efficacy in certain forms of constipation as well as of dysenteric diarrhœa is well known.

Let us now, for a moment, examine a little more closely the nature of the functions affecting which the alternate series of opposed conditions, which have been called primary and secondary, are mostly observed in drug-proving. 1st. They are such as in the nature of things are periodic, and not continuous; characterized by periods of repose and activity, and susceptible of quantitative and qualitative correlative interchange among themselves. Thus sleep is periodic, and capable of being supplemented to a degree by other forms of repose to the nervous system. The intestinal canal, the genito-urinary apparatus, the skin, in so far as secretion and excretion are concerned, have periods of activity and repose; and the inactivity of one may be made up by increased activity of another. And thus the function of any one of these apparatus may vary widely at different times without a condition of opposition being established. For this reason, then, the mere quantity of one of the excretions, or the degree in which any one of these periodic and convertible functions is performed, does not rank first among the indications on which the selection of a drug is to be based. If we now analyze the prescriptions of Veratrum and Nux vomica referred to, we shall find certain constant phenomena characterizing both the constipation and the diarrhœa, and which would determine the prescription almost without reference to the excretion.

The Veratrum diarrhœa is uncontrolled and almost unnoticed by the patient, liquid fæces escaping with the flatus. Here we have a paretic and anæsthetic state of the rectum and sphincter. The Veratrum constipation exists solely because the rectum does not perform its expulsive function, and is not, as normally it should be, irritated thereto by the presence of fæces. Here likewise is a paretic and anæsthetic condition. But Veratrum is not fully indicated in either case without the characteristic general symptoms: general depression of vitality; predominant coldness of the body; pallor and cold sweat of the forehead, or of the whole body, on

slight emotion or exertion, as, for example, on having a diarrhœic stool, or making the ineffectual effort to have a stool, if constipated.

Both the constipation and the diarrhœa of Nux vomica are characterized by increased but uncoördinated activity of the intestine, evinced by tormina and tenesmus, and frequent insufficient stools; so that the condition of intestinal action is the same, whether there be, as in one case a *minus*, and as in the other, a *plus* of excretion; and, indeed, in the Nux vomica patient these conditions often alternate. These remarks and instances will sufficiently illustrate my conclusions, viz.:

That the appearance or non-appearance of opposed series among the symptoms of a drug depends chiefly, if not altogether, upon the dose in which the drug was proved; and that the question of the constant and necessary appearance of such series cannot be determined until experiments with a uniform and the least possible dose shall have been made by many provers with the same drug, and in the case of many drugs, and therefore that, 1st, Although in our Materia Medica, as it now exists, pathogeneses do present certain series of symptoms more or less opposed, nevertheless (excluding the symptoms of the agony which are not available in practice), inasmuch as these series of symptoms occur in different orders in different provers according to dose or idiosyncrasy, no sound practical distinction can be drawn between them, based on assumed difference of nature, by virtue of which they can be designated respectively as primary and secondary.

2d: That symptoms, apparently opposed (not including those of the agony), occurring in a drug-proving are equally available as guides in the selection of remedies.

Coming now to the special subject of this paper, I justify the length at which the preliminary subjects have been discussed by the suggestion that, if I have shown that there is no basis for a division of drug-symptoms into primary and

secondary, I have thereby shown the impossibility of a law of dose based on such a division. Or, if admitting that in pathogeneses there do appear groups of symptoms apparently opposed, I have shown that these refer only to certain functions, and by no means embrace, or could be made to include, the symptoms of the entire organism. I have thereby shown that an alleged law of dose based on the existence of these groups, must necessarily be partial, and therefore devoid of that generality of application to the entire pathogenesis which alone would justify the appellation "LAW;" and I claim to have shown these things.

In 1844, Dr. C. Hering in a letter to the German Central Verein in session at Magdeburgh (*N. Archiv.*, *21*, 3, 166) rejects Hahnemann's explanation of primary and secondary symptoms. He admits that there are in every proving primary and secondary symptoms, in the sense that some symptoms appear earlier and others later in the course of the proving, but affirms that although these may appear to be opposed to each other, they are all to be regarded as drug-symptoms, and as such are to be made the basis of prescriptions. He gives instances of such prescriptions in the cases of Conium, Opium, and Mercury. Indeed, he says that the longer-lasting, more permanent and more opposed to the earlier these later symptoms are, the more useful are they in practice. He says further, that "the course of the drug-disease (in proving) must correspond with that of the disease to be treated." Hering further states that "all symptoms which arise in provings of the higher potencies are similar to the later effects of the lower or so-called stronger doses, and are not like the *first* effects of strong doses." He adds that "the great characteristics of remedies [as we have seen in the cases of Nux vomica, Veratrum, etc.] accompany both the earlier and later symptoms, *e. g.*, the *burning* of Arsenic."

From these considerations, Hering deduces the following law of dose:

"Having chosen the remedy according to the symptoms of a case from the complete correspondence of the charac-

teristics in disease and drug, we have only to consider whether the symptoms of the case generally have more resemblance to the earlier (primary) symptoms of the drug, and then we give the lower potencies; or more resemblance to the later (secondary) effects, that is, to the symptoms produced by the higher-potency provings, and then we give the higher."

It may be said that this is simply saying: Prescribe doses analogous to those which produced, in the proving, the symptoms presented by the case under treatment. But it amounts to a great deal more, for by showing that the symptoms produced in provings by small doses correspond with the later effects of large doses, it enables us to infer the effects of small doses in cases where provings have been conducted with large doses only.

In 1860, Dr. E. M. Hale published an elaborate article in the *N. A. J. of H.*, vol. ix., on "The Dose," in which he expresses the belief that he has discovered the long-desired law by which "the proper dose for each case may be selected with as much certainty as the proper remedy," as follows:

"In any case of disease we must select a remedy whose primary and secondary symptoms correspond with those of the malady to be treated," and

"If the primary symptoms of a disease are present, and we are combating them with a remedy whose primary symptoms correspond, we must make the dose the smallest compatible with reason; and if we are treating the secondary symptoms of a malady with a remedy whose secondary symptoms correspond, we must use as large a dose as we can with safety."

These propositions rest on the assumption that all drugs produce, and all diseases present, two series of symptoms, primary and secondary, and that in one or other of these classes is embraced every symptom of drug or disease. I have expressed my dissent from this view, and given reasons for thinking that a distinction between primary and second-

ary symptoms, if ever justifiable, is, at most, applicable to but a portion of the symptoms of each drug. It cannot, if I am correct, be made the basis of a *general* law.

Dr. Hale illustrates his meaning by referring to Aconite, the primary symptoms of which, he says, "correspond perfectly with the *chilly stage of all fevers*," while the secondary symptoms of Aconite "correspond with the hot stage of fever." He therefore recommends a small dose of a dilution of Aconite during the chilly stage, and large doses of the tincture or 1st decimal during the hot stage, and relates results of such treatment, which seem to have satisfied him, but would hardly have satisfied me.

Again, he tells us that the primary symptoms of a group of remedies, of which Cinchona, Ferrum, Conium, Nux vomica, and Ignatia are members, are such as to denote that these drugs first "increase the tone and strength of the muscular or nervous system, impart vitality and vigor to the functions of the vital organs;" and that their secondary effects are to cause "a peculiar atony, a condition of deficient vitality, and a cachexia of a more or less obstinate character." And, stating further, that an outbreak of intermittent fever is always preceded by "an era of good feeling," a condition of exalted muscular and nervous tone and vigor, he regards this as the primary stage of that disease, to be treated, when recognized, therefore, by small doses of Cinchona. The outbreak of paroxysmal fever is regarded by Hale as belonging to the secondary series of symptoms, and therefore to be treated by as large doses of Cinchona as may safely be given.

In a paper presented to this Institute in 1874, Dr. Hale reiterates these views in substance and re-affirms this alleged "law of dose." He illustrates the two alleged series of symptoms in disease, called primary and secondary, by the instance of inflammation of the urinary passages, in which congestion and arrested secretion with fever and pain appear first, and are succeeded by profuse secretion or suppuration,

prostration and cessation of pain, etc. And he affirms that the group of cognate drugs, of which Cantharis may be the representative, presents similar series of primary and secondary symptoms.

Considering, first of all, the last illustration, it is true that the process of inflammation consists of several successive steps, "accelerated and increased circulation, followed by retarded and diminished circulation, exudation of liquor sanguinis, emigration of leucocytes and stasis," and it is true that the earlier steps are prior to the later, but they are equally successive and necessary steps in a uniform and definite process, and nothing justifies the drawing a line anywhere in the process, and affirming that all that lies behind the line belongs to a different and opposite series to that which lies in front of it. Nor can I recognize in drug-provings any such opposed series. Neither Hahnemann's provings of Cinchona and Ferrum, nor Hahnemann's and Harley's proving of Conium, nor Hahnemann's provings of Nux vomica and Ignatia seem to me to support the allegations of Dr. Hale.

Alluding now to Hale's remarks upon Aconite, I refer to my quotations of Hahnemann, Rothausl, Zlatarovich and Sharp, which show that Aconite does not produce a distinct chilly stage first, followed by continued heat, but, as Hahnemann says, "alternating conditions made up of shivering or cold and heat, and recurring frequently or at longer intervals." So that while I will not deny that Dr. Hale prescribed as he says he did, and with the results which he describes, I do affirm that the pathogenesis of Aconite affords no ground for the theory on which he says he acted, or which he deduced from his action.

And, if I may deviate for an instant from the subject strictly under discussion, these statements about Aconite and its application suggest the fact that many prescriptions professedly homœopathic are really made *ex usu in morbis*, and are not based on a strict individualization of the case, and a

selection of the drug whose symptoms correspond to it. Aconite, having early acquired a reputation as a remedy in febrile conditions, is too often given in routine fashion, "in all fevers" (to use Dr Hale's phrase), and therefore in many cases in which the symptoms do not at all indicate it. For, as even Hahnemann warned us, "it is not in every case presenting febrile symptoms that Aconite is homœopathically indicated;" and therefore it cannot truly be said to correspond to the "chilly" or any other "stage of all fevers." Given when the patient is quiet, lies still, is tranquil in mind and hopeful or patient, it will not remove the symptoms as a homœopathically selected remedy would do. Nor is it indicated by a similarity of symptoms in any save a small minority, if in any, of the intermittent, remittent or continued fevers which so often prevail in different parts of our country, nor in the febrile condition which persists when parenchymatous inflammation is established in any important organ.

It was my purpose to follow and analyze the statements and arguments of this writer on this important subject, but I find that I cannot do so. His premises consist of citations from allopathic writers, which seem to me to be statements based on observations of the action of drugs in large doses on the sick, and on theoretical deductions from these observations, and which certainly bear little or no resemblance to the pathogeneses of the *Materia Medica Pura*, on which I have been wont to rely for a knowledge of drug-effects, but to which my colleague rarely refers. His allusions to treatment seem to me to be of a very generic character, based on assumptions of the pathological nature of the disease in question, and consisting of an application of drugs according to a vague and general resemblance of assumed pathological conditions. In all of this I fail to see any allusion to, or any place for, the strict individualization of cases, which is the very essence of sound homœopathic treatment. In studying these papers, in fact, I hardly realize that I am perusing the

works of a practical homœopathist, according to my conception of Hahnemann's idea of homœopathic practice, and I perceive, to my dismay, that I do not stand on common ground with the author to the extent necessary to make possible a further discussion of his treatment of the question at issue.

I must content myself, therefore, with the simple statement that my own observation and experience do not enable me to corroborate Dr. Hale's statement of the "Law of the Dose," based on the distinction of primary and secondary symptoms Where Aconite has been truly indicated by the symptoms of the case, I have seen prompt relief follow the administration of a high potency, given when the patient was in the hot stage, and likewise when in the chilly stage; and in both the effect has appeared in a much shorter time than Dr. Hale's remarks would lead one to anticipate.

In the treatment both of constipation and of dysenteric diarrhœa by Nux vomica in cases in which the characteristics of Nux vomica were present, the higher potencies have been equally efficient, leaving nothing to be desired, and the same may be said of Veratrum album in diarrhœa and in constipation, when the characteristics of that drug were present.

It may be, and I am inclined to believe, that the law proposed by Dr. Hering may be found to represent the facts; but for its demonstration and its general application in practice, we need a much more complete Materia Medica than has yet been furnished us.

In conclusion of this branch of the subject, I think that no law for the determination of the dose can be deduced from the relation of opposition or contrariety on the basis of which symptoms have been divided into series of *primary* and *secondary*.

THE DOSE IN DRUG-PROVING.

The symptoms which drugs produce upon the healthy organism vary according to the dose. They may be:

1. CHEMICAL—depending on the chemical affinity which exists between the drug and the tissues of the body, and independent of vitality; or,

2. MECHANICAL (or revolutionary), consisting chiefly in violent efforts on the part of the organism to eject from its cavities the offending substance; or,

3. DYNAMIC, contingent upon vitality and resulting from the relations of the peculiar properties of the drug to the susceptibilities of the living, healthy organism. These *dynamic* effects may be:

A. *Generic*—such as are common to all the members of a certain class of drugs and which serve to distinguish this class from others, but do not furnish means of distinguishing between different individuals of the same class. For example, Arsenic in certain doses produces vomiting and diarrhœa, with cold sweat and cramps of the extremities. These are *dynamic* effects of Arsenic but they are *generic*, since other members of the class to which Arsenic belongs, viz.: Cuprum, Veratrum, Tartar emetic, etc., in certain doses, produce identical symptoms; and were these remedies proved in such doses alone, it would be impossible to distinguish the pathogenesis of one of them from that of any of the others.

B. *Specific*—such as results from the dynamic action of the drug and are peculiar to it. They serve to distinguish a given drug from all others. For example, Arsenic, taken

in different doses from those which produce the generic dynamic effects, produces vomiting and diarrhœa or tendencies thereto; but these phenomena are accompanied and characterized by conditions quite different from those which accompany the similar symptoms of Cuprum, Veratrum, etc., and are thus distinguishable from the effects of these drugs.

The *Specific-dynamic* symptoms may be again sub-divided into *Central* and *Peripheral.*

The *Central* symptoms appear speedily after the drug is taken, are generally the result of comparatively large doses and, in the case of many drugs, are confined to the alimentary canal and to the organs immediately connected with it.

The *Peripheral* symptoms appear more tardily, are generally the result of comparatively small doses, taken repeatedly or allowed to act without interruption for a long period, and appear in the bones, skin, glands, etc., and in the coördinated phenomena of life. They are often the manifestations of a dyscrasia or cachexy. Doses which produce central symptoms do not generally produce the peripheral (or at least not until after a long period has elapsed) and *vice versa.* For example, Mercury, in certain doses, produces well-marked and characteristic action upon the alimentary canal and its appendages. In smaller doses it produces, instead of these effects, a series of symptoms in the skin, bones, glands, etc.—the Mercurial cachexy. The former are what we mean by *central* specific dynamic symptoms. The latter are the *peripheral* symptoms. Arsenic, again, furnishes, according to the dose, examples of all of the above varieties of symptoms. In certain doses it develops chemical and revolutionary effects. In smaller doses, as we have seen, *generic dynamic;* in still smaller doses, *specific dynamic* symptoms of the *central* variety. In yet smaller doses, it produces *peripheral* specific symptoms, which are those of the so-called "gradual poisoning;" as for instance in poisoning by exhalations of Arsenic from green wall-paper, in which the phenomena of vomiting and diarrhœa

or the *central specific* symptoms do not appear, but instead of these we have evidence of a distinct cachexy, in the skin and glandular symptoms, marasmus, etc.

Such are the varieties of symptoms produced by corresponding varieties in the *dose*. It is hardly necessary to say that they are not always to be distinguished with precision; but the facility with which we are able to recognize them is in proportion to the completeness of our proving.

It unquestionably behooves the homœopathic physician to have an exhaustive knowledge of the whole sphere of action of his drugs; but, as a *prescriber*, he *must* be familiar with the varieties and subvarieties of *dynamic* effects which we have specified. This knowledge is to be attained in the first place only by drug-proving. The proving of drugs must then be so conducted as to produce in the greatest possible completeness and clearness, each of these varieties and subvarieties. This, as has been shown, is to be accomplished by a skillful selection and succession of doses. It is not so simple and easy a matter as it might at first view appear to be: for,

First: The doses by which the corresponding varieties of symptoms are produced, differ widely in different drugs. For example, a half grain of crude Nitrate of silver or of Sulphuric acid produces chemical symptoms, while a half grain of Lycopodium or of Silicea produces probably no symptom at all. A grain of Arsenic produces *generic* dynamic symptoms, while ten grains of Natrum muriaticum may be inert. Forty drops of Bryonia tincture may excite a fair show of *specific dynamic* symptoms, while forty drops of tincture of Opium will produce *generic dynamic* symptoms or full narcotism.

Secondly: The susceptibility of different provers to the same drug is very different, and the degree of susceptibility which each prover possesses is to be learned only by experiment. For example, one prover will take five hundred drops of Thuja without any effect; another, taking twenty drops, experiences violent *specific* symptoms.

Thirdly: The susceptibility of provers to different preparations of the same drug is very various and apparently capricious. One records characteristic specific symptoms from large doses of the crude drug, and is not affected by smaller doses; another is acted on by dilutions and not by any quantity of the crude substance.

The relative power of a drug and susceptibility of the prover being altogether unknown until ascertained by direct experiment, the proving of a new drug is therefore a matter of pure experiment in every particular, and it might at first view be supposed to be a matter of indifference in what manner or with what doses the experiment is begun; which variety or subvariety of symptoms is first developed, whether we take heroic doses and develop chemical symptoms or small doses and produce peripheral dynamic symptoms; since in either case we should be able by subsequent experiments based on the first, to develop the complementary symptoms and thus complete our proving. Experience teaches, however, that this supposition is not sound, and for the following reasons: Drugs vary not more in the intensity than in the permanence of their action upon the organism. Some drugs appear speedily to exhaust, sometimes by a single large dose, the susceptibility of the prover, so that no subsequent doses, whether large or small, produce any effect. Of others again, a single large dose develops some one *generic* or *central specific* symptom, and along with it induces such an exalted and distorted susceptibility that every subsequent dose, whether large or small, evokes straightway that one symptom or series of symptoms and none other. Thus the proving is in either case partial and incomplete—we fail to get those symptoms which are the most valuable of all to us, as being those which clearly characterize the drug and enable us to distinguish it from all other drugs, viz.:—the *peripheral* and *central specific* dynamic symptoms. To illustrate this point,—it is well known that Mercury given in such doses as to produce *central specific* symptoms, induces often so great a susceptibility

of the organism to the action of this drug that subsequent doses, even of tolerably high dilutions, provoke straightway a series of *central* symptoms. The same is true of Arsenic. We have seen a case in which, generic and specific symptoms having once been produced by massive doses of Tartar emetic, the organism remained so sensitive to the action of this substance, that a few globules of the thirtieth dilution would at any time produce vomiting and diarrhœa, with cold sweat and prostration. It may be said that these are cases of very unusually great susceptibility to the action of the respective drugs. This is true, but it is precisely such cases of great susceptibility that are of exceeding value to us, for in them, by judicious experimentation, we could get most valuable peripheral symptoms, unalloyed by generic or by revolutionary effects.

There is no reason to believe, on the other hand, that small doses, so administered as to produce the peripheral specific symptoms, modify the susceptibility of the prover in any such way as to prevent his obtaining by subsequent larger doses the central specific, the generic dynamic, or even the chemical and mechanical effects. It follows from what has been said, that to obtain an exhaustive proving of a drug, we should begin with small doses, gradually increasing the quantity until unequivocal symptoms appear. We shall thus, if we continue our experiments a suitable length of time, obtain peripheral symptoms; and these small doses will not have so influenced the system as to prevent our obtaining by subsequent larger doses the other varieties of effects. Inasmuch as, in the nature of things, the peripheral symptoms, representing, as they do, a cachexy, cannot be *speedily* produced, a considerable space of time should be devoted to our first experiments with small doses. Finally, after an interval of non-medication, larger doses should be taken until we have exhausted the whole dynamic action of the drug, and even obtained a fair picture of its chemical and revolutionary action, although this may in a measure be gained from records of *poisonings*.

But, in this relation, what are the "small doses" with which we are to begin our proving? The term is comparative. Are they drop doses of the tinctures, or are they high dilutions? They are such doses as have, in the proving of some previous drug, shown themselves capable of producing unequivocal symptoms. We must search the records of provings, therefore, for our standard initial dose. What this is at present we shall soon see: as our experience increases, this standard may from time to time be altered.

It is evident that the method of conducting a proving is a matter of great importance, and should not be left to caprice or accident. The completeness of our Materia Medica, and consequently our ability to cure disease, depend upon our selection of a happy method. This important subject has received the attention of the American Institute of Homœopathy, to which the Central Bureau of Materia Medica has presented a report on Drug-Provings. The majority of the Bureau repeat Hahnemann's directions for proving as contained in the *Organon;* and as regards the dose, they recommend "the prover who makes use of potencies" to make a trial of the high potencies first, and afterward, if necessary, to take the lower dilutions and triturations, or the crude substance or tincture, if satisfactory results are not obtained with the attenuations.

This recommendation accords with our deductions and corresponds with the spirit of Hahnemann's directions. Hahnemann's instructions differed at different periods of his life. One essential idea, however, pervades them all—a small dose is to be taken at first, and the dose is to be increased until unequivocal symptoms manifest themselves. In the last edition of the *Organon* he adds, as the result of his extensive observation, that "The most recent experience has taught that medicinal substances, when taken in the crude state, do not for a long time display the full extent of their virtues, as they do when taken in higher developments. Thus any one, even of those medicines whose virtues are

considered weakest, is now found to be most advantageously studied if four to six globules of the thirtieth dilution be taken every morning for several days." In this statement, Hahnemann does not contradict the spirit of his former directions, for he adds, "Should the effects of such a dose be weak, it may be daily increased." He further adds, "The more moderate the dose, the more are the primitive effects developed, which are the most important to be known." We see nothing in Hahnemann's writings which shows that he ever thought of restricting the dose in proving to the thirtieth dilution, as some have stated; he simply assures us that unless provings with so high a dilution were made, the prover would fail to get all of the symptoms which the drug is capable of evolving.

In the Minority report of the Central Bureau,[1] Dr. Hempel differs from the majority in so far as the proving with dilutions is concerned. He would propose that "all such provings should be rejected rather than encouraged;" and he "is convinced in his deepest soul, that it is owing to the incorporation of such provings in our Materia Medica that all the confusion and uncertainties with which it is now tainted, are presenting immense and almost insufferable difficulties to the inexperienced student of our science." We trust that it is unnecessary to say that in commenting freely, as we shall do, on Dr. Hempel's report, we are actuated by no unworthy feeling toward our colleague, whose unceasing and very arduous labors in the cause of Homœopathy command our highest respect, and deserve the grateful recognition of every English and American Homœopath. We speak of him only because, by his report, he stands forth as the representative of certain opinions, which seem to us unsound and unsupported by the evidence on which they are supposed to rest.

His report may be reduced to the following propositions:

1. Drugs should not be proved with attenuated substances.

[1] *Am. Hom. Review*, vol. i., p. 575.

2. The middle and higher potencies do not produce reliable symptoms, unless the system has been previously saturated with massive doses of the original drug. Corollary: the saturation of the system by massive doses of a drug renders it susceptible to the action of the middle and higher potencies.

3. In exceptional cases a peculiar idiosyncrasy may enable the organism to develop symptoms from the higher potencies; *but* "it is unreliable to commence the proving with these potencies." The sequence of this conclusion is not very clear. We suppose Dr. Hempel to mean that symptoms developed, where an idiosyncratic susceptibility to drug-action exists, are not so reliable as those developed where there is no idiosyncrasy.

It is much to Dr. Hempel's credit that he has not left these propositions to stand in his report as bare unsupported assertions. Recognizing the experimental nature of the question, he has referred for corroboration of his views to the experience of those who have made our provings, and has called to the witness-stand the great body of our drug-provers in the following terms: "*All* the splendid provings of the original provers of the Materia Medica, and of the Austrian Provers' Union, of the Provers' Society of Prague, and of any other Provers' Society, whose provings are accepted with universal acclaim and confidence, have been instituted with massive doses of the strongest preparations of the drug; the higher and middle potencies were *invariably* tried *after* the former." If this statement were literally correct, if, indeed, none of these provers *ever began* their provings with potencies, then they are incompetent witnesses for Dr. Hempel's purpose—they are incapable of testifying as to the action of dilutions when *not* preceded by massive doses, since, Dr. Hempel says, *they never tried them.* If this were so, then the second proposition of Dr. Hempel's report would rest unsupported save by the one witness whom, we neglected to say, Dr. Hempel cites, first of all, in

these words: "Dr. Hempel has never been able to elicit any reliable symptoms by means of the middle or higher potencies, unless the organism had been previously saturated with massive doses of the original drug." On this one point of negative evidence, then, this important proposition stands—"Dr. Hempel has never been able." But perhaps *others have* been able. Perhaps the very witnesses whom Dr. Hempel has called have been more successful than he. We will cross-examine them.

First, then, we call upon the "Original Provers of the *Materia Medica*," by which we suppose Dr. Hempel means Hahnemann's "Materia Medica Pura" and "Chronische Krankheiten." Of these provers, the "Great Original" was Hahnemann himself. His pupils and friends adhered strictly to his directions and method. What Hahnemann's opinion was as to the propriety of commencing a proving with small doses we have already seen. After thirty-five years' experience in drug-proving, he sums up his observations in the advice to begin with the thirtieth dilution. But what was his *practice?* What doses did he actually take? It has been generally supposed that he did not as a rule record the doses with which his provings were made. Dr. Hempel, however, seems to have had access to some sources of knowledge on the subject that are not open to the general reader, for he tells us without qualification, that "*all* the splendid provings of the original provers of the Materia Medica were made with massive doses of the strongest preparations, etc." Those who have not enjoyed these unusual means of information, gather from a few observations, scattered through Hahnemann's writings, the following facts:

Silver was proved by Hahnemann in the first trituration. The *Nitrate of Silver*, of which he gives a few symptoms, in the fifteenth dilution. *Carbo vegetabilis* was proved in the third trituration. In a letter in the Neues Archiv. (1813), he directs Stapf to prove Helleborus thus: "Add a drop of the tincture to eight ounces of water and one drachm of

alcohol; shake well, and take an ounce every hour and a half or two hours until some decided effects are produced." And camphor, thus: "Dissolve two grains in a drachm of alcohol; shake this well with eight ounces of water, and take in from four to six doses during the day."

In the first publication of the proving of Natrum muriaticum (1830), Hahnemann tells us that a great part at least of this proving was made with the *thirtieth dilution*, and he adds, that "it is only in such a highly potenized form that this and all other drugs display the *whole* of their power to alter the condition of the organism." This was Hahnemann's conclusion after thirty years of active experience in drug-proving. As a voucher he gives us the proving of Natrum muriaticum, the value of which is attested by the clinical experience of the last thirty years, and confirmed as we shall see by the Austrian re-proving. In the same volume of the Chronische Krankheiten, Hahnemann published the proving of Kali carbonicum, and large additions to the previously published provings of Carbo vegetabilis, Causticum, Conium and Sulphur. It is but reasonable to conclude that the symptoms of Kali carbonicum, and many at least of those of the other drugs above named, were produced by the thirtieth dilution as well as those of Natrum muriaticum.

In view of these facts, it is hardly correct to say, as Dr. Hempel does, that "all the provings of the original provers of our Materia Medica were made with massive doses of the strongest preparations, etc."

Second, we call upon the Austrian Provers' Union. This society conducted in the years 1842 to 1848, a series of re-provings of certain of the drugs proved by Hahnemann, in the hope of discovering the pathological connection of the symptoms which in Hahnemann's scheme are disconnected. Conceiving also that some of Hahnemann's symptoms were vague, because produced, as they supposed, by *small* doses, they pushed their provings with massive doses, in many

cases to extreme poisoning. With these views we could hardly expect from them much testimony, either positive or negative, on the subject of proving with potencies; but, in that which they do furnish, we should expect them to be unfavorable witnesses for our views, since their proclivities are almost uniformly against the use of potencies in proving or in therapeutics. Dr. Hempel says, "All these splendid provings, etc., were instituted with massive doses; the middle and higher potencies were *invariably* tried *after* the former." Let us examine the records.

In the proving of Aconite, sixteen provers made in all thirty-seven experiments. Six of these were made with dilutions from the first to the twelfth centesimal, and symptoms were observed in all but one. In five of these cases, large doses of the *tincture* had been taken *before* the dilution. In one case the prover began with the first centesimal dilution and got symptoms.

In the proving of Bryonia, fourteen provers made twenty-nine experiments, of which ten were with dilutions from the first decimal to the 203d. In two of these, the provers began with the dilutions, viz.: with the tenth and the thirtieth, and got in one case very graphic symptoms; in the other, symptoms unequivocal though less numerous. In the cases in which dilutions were used after massive doses of the tincture had been taken, considerable intervals were allowed to elapse after the last symptoms from the tincture-proving disappeared before the dilutions were taken, *e. g.*, forty-one days before the 203d dilution was taken, and yet this dilution appears to have produced decided symptoms. It is true that the propriety of ascribing these symptoms to the Bryonia is called in question by the Editor of the Austrian Journal, but with scarcely sufficient grounds, since a subsequent proving with the 203d dilution evoked them again. Here, then, in the only two cases in which dilutions were taken before the tincture, symptoms were obtained (while in one case in which they were taken *after* the

tincture, *no symptoms* were observed, shows that the corollary to proposition No. 2 is at least not *universally* true).

In the proving of Silver we find that the drug was taken by one prover each in the first, second, and third triturations, and in the fourth, fifth, and sixth dilutions, and valuable symptoms were obtained by each. The Editor calls attention to the great correspondence between these symptoms and those of Hahnemann, obtained, as we know, from the first trituration.

In the proving of Nitrate of Silver, the Editor remarks: "Between the experiments a sufficient time elapsed to insure against the effects of one experiment being complicated by those of a preceding one, two years being consumed in the proving." The proving was begun with the first decimal trituration, and subsequently in succession the second, sixth, and thirtieth dilutions were used. The effect of each dose was allowed to expend itself before a new experiment was instituted; symptoms clear and characteristic, and covering a period of six to nine days, were obtained from a single dose of the thirtieth dilution. Here, it is true, the dilutions were preceded by low triturations, yet the Austrian provers were of opinion that in these cases the lower preparations did *not* influence the susceptibility of the system to the higher potencies.

In proving Kali bichromicum, in only two cases was a dilution higher than the third employed. In both of these the twelfth was used and symptoms were obtained. In one, that of Dr. Wachtl, the symptoms from a single dose give a complete picture of the subjective and objective effects of this drug in the pharynx and neighboring organs. In neither of these cases had the drug been previously taken in massive doses.

In the proving of Colocynth, fifteen provers made twenty-five experiments. Ten were made with dilutions, and in nine, symptoms were obtained. In three of these, the third dilution was used and no other preparation of Colocynth had previously been taken.

In proving Thuja, twenty-three provers made fifty-nine experiments. Seventeen were made with dilutions, and, in thirteen, unequivocal symptoms followed. Nine began their provings with dilutions and got symptoms. Four began their provings with dilutions and got *no* symptoms. But, on the other hand, seven conducted their provings with massive doses of the tincture without obtaining any unequivocal symptoms. It is evident, then, that negative testimony on this subject is of little value.

In proving Cochineal, four provers began their experiments with the hundredth dilution, one with the sixtieth and one with the thirtieth dilution, and all got valuable symptoms. One prover, on the other hand, began with the 200th dilution and gradually descended, getting no symptoms until he took massive doses.

In the proving of Natrum muriaticum, thirty-nine provers took part. Fourteen began with dilutions varying from the sixth to thirtieth,—the majority beginning with the thirtieth,—twelve obtained symptoms of which some are of great value, lasting and recurring for many days after the last dose. Thus, in twelve cases out of thirty-nine, dilutions were taken before "the system was saturated with massive doses of the strongest preparations, etc.," instead of dilutions being "*invariably* tried *after*" such massive doses.

We may remark that in every case in which we ascribed symptoms to the dilutions, except the case of the 203d of Bryonia, they have been so admitted by the Austrian provers themselves.

In conclusion, we may observe that with Natrum muriaticum the record of the Austrian provings closes. In his concluding remarks on that drug, Dr. Watzke uses the following language: "I am—alas!—I say, alas! for I would much rather have upheld the larger doses which accord with current views—I am compelled to declare myself for the higher dilutions. The physiological experiments made with Natrum muriaticum, as well as the great majority of the clinical

results attained therewith, speak decisively and distinctly for these preparations. *Several of our most cautious* provers have obtained unquestionable characteristic Natrum muriaticum symptoms from them." It is remarkable that as Hahnemann was evidently led by experience to recommend that in drug-provings dilutions should be used as well as crude preparations, in like manner, the Austrian prover should, as the result of his experience, express himself as above. Watzke further remarks:[1] "I am very far from regarding a *stormy* method of proving as the only proper and useful one, and I recognize the conditions of an all-sided knowledge of the remedy, in the free, unrestrained method we have adopted; experiments being made with *large*, *small*, and *smallest* doses according to the disposition, judgment and good pleasure of the prover." And again, in reply to a critic, "My friend says, '*I* have proved all *my* remedies in strong and some in the strongest doses; with many I have also made provings with the higher, even with the thirtieth dilution'—Have *I* then done otherwise?"

In a recent number of the *Allg. Zeitung*, it is stated that a member of the Old Austrian Provers' Union has begun to prove Quassia amara with the thirtieth dilution, and has obtained valuable symptoms. *Experto crede.*

In view of these facts, we think Dr. Hempel is hardly correct in stating that "*all* the provings of the Austrian Provers' Union were made with massive doses, etc.—the middle and higher potencies being *invariably* tried *after* the massive doses."

The records of the Austrian Union not only prove that dilutions may evoke trustworthy symptoms in a majority of the cases in which they are employed, but they also give us the means of forming a judgment on the propositions which form the substance of Dr. Hempel's report.

1. "Drugs should not be proved with attenuations."—Now, since it has been shown that attenuations produce

[1] *Oest. Zeitschrift*, vol. iii., p. 64, note.

reliable and valuable symptoms, we submit that they should be used and for these reasons:

(*a.*) Because attenuations have produced symptoms which have not also been produced by massive doses, and which have been confirmed by clinical experience. In the proving of Natrum muriaticum, the only distinct picture of periodic fever was induced in Dr. Wurmb while proving the thirtieth and twenty-fifth dilutions. Dr. Schreter, proving the thirtieth, got symptoms which no prover with massive doses presents. In the proving of Thuja, certain symptoms affecting the vision were produced in Dr. Huber by the sixtieth dilution, and in Prof. Zlatarovich by the twelfth, which no other provers have recorded. In the proving of Colocynth, the third dilution produced symptoms not recorded by any prover who took massive doses, and yet corroborated by a second prover with the third.

(*b*) Because some provers are susceptible to the action of dilutions or small doses, and not to that of massive doses of the crude substance. For instance, Dr. Wurmb got decided and persistent symptoms from dilutions (thirtieth to sixth) of Natrum muriaticum. From lower dilutions and from the crude substance, even in half-ounce doses and repeated frequently, he got no symptoms. Was this because his system was saturated? We think not; for on recurring to the dilutions, he again got symptoms as at first. Now, had the provings been confined to the crude substance, we should not have Dr. Wurmb's proving of Natrum muriaticum, the most complete and reliable single experiment that we possess, and which we could hardly consent to give up, notwithstanding Dr. Hempel's opinion that "potency-provings should rather be rejected than encouraged." The same thing is observed in the provings of Aconite, Thuja and Colocynth.

2. "Potencies do not produce reliable symptoms unless the system has been previously saturated with massive doses of the crude substance; and, Corollary, massive doses

render the system susceptible to the action of smaller quantities."

We have shown that in repeated instances, symptoms which Hahnemann and the Austrian provers regarded as "reliable" have resulted from potencies not preceded by massive doses—symptoms confirmed by other provings and by clinical experience. On what grounds does Dr. Hempel question their reliability?

Again, we have no conclusive evidence that massive doses, as a rule, render the system more sensitive to small doses. In some cases they appear to do so. In others they have the opposite effect. In the records, we find the utmost variety as regards the susceptibility of provers to various doses. Some, like Dr. Wurmb, are susceptible to dilutions alone of a given drug, others to massive doses alone. Many obtain symptoms at first from comparatively small doses of the crude substance, but find it necessary to steadily increase the dose, until finally, after taking in some cases several hundred drops or grains, they get no symptoms at all from any preparation. This looks like a *blunting* of the susceptibility rather than an exaltation. We are not always justified in assuming that, where dilutions were taken *after* massive doses, the symptoms were due to the massive doses and not to the dilutions. In the majority of these cases a sufficient period elapsed between the experiments to prevent all possibility of the one complicating the other. In a few cases no such period elapsed, and in these it may be that the symptoms ascribed to the dilutions were really due to the previous massive doses, yet even in these cases we cannot be certain of this; for in one of them, that of Prof. Zlatarovich, who, after taking in the aggregate several thousand drops of the Thuja-tincture, changed without delay to the twelfth dilution, a symptom was observed which had not occurred to him or to any one else while proving the *tincture*, but only to *Huber while taking the sixtieth.*

In conclusion, we see in the Austrian records more reason

to suppose that massive doses, as a rule, *blunt* than that they exalt the susceptibility, although in some cases they unquestionably have the latter effect. As these cases cannot be recognized in advance, it is clear that in the case of an individual drug or prover, nothing positive on this head can be assumed as a basis on which to plan a proving.

3. It is said that "*idiosyncrasy*" determines the action of potencies, and that provings having this origin are not reliable.

The word "idiosyncrasy" is used in two senses. Properly, it signifies an abnormal sensibility to drug-action; or such a relation to a special drug as that the drug produces in the prover symptoms that occur to no other prover, and which bear no analogy at all to the symptoms which others experience. Such symptoms as these must always be regarded as suspicious until verified by clinical experience. Such an idiosyncrasy is exemplified by persons who have "hay asthma," "rose catarrh," etc.

But, as most frequently employed, the word "idiosyncrasy" is synonymous with "*susceptibility*," implying an *unusually acute*, but not *abnormal*, sensibility to the action of the drug, and a prover who shows a marked susceptibility to the action of a drug is said to have an idiosyncrasy which favors its action. This susceptibility is similar to that which individuals exhibit for natural diseases—some being prone to one kind of disease, others to another, just as one prover is especially susceptible to Thuja, another to Aconite. Now, what pathologist would think of objecting to a case of pneumonia that it was not a reliable picture of the disease, because it could be shown that the patient had a great susceptibility to the disease, and took it on the slightest provocation? A certain susceptibility to the action of a drug is absolutely necessary to a good proving. Hahnemann speaks of it in his *Organon* as a necessary condition. What the Austrian provers think on the subject may be gathered from the following remarks of Watzke (Dr.

Wurmb proved Thuja five times,—four times in massive doses and once in dilutions; result very meager): "We may observe that the proving of a drug is not so easy an affair as it seems to be. The individuality of the prover as well as of the drug plays here so important a part, that a successful proving is often an entirely accidental piece of good fortune of the susceptible prover, while the boldest, but unsusceptible prover, with the best good-will and the greatest devotion may obtain only a scanty and one-sided result."

This susceptibility may be to the drug in general or to particular preparations of it. Wurmb was acted on only by dilutions of Natrum muriaticum. Watzke only by immense doses of the crude substance. Watzke was sensitive to very small doses of Thuja, while Wurmb and Reisinger were almost insensible to *every* preparation of it. Obviously, then, a prover's susceptibility varies with the drug and with the preparations, and can be learned only by experiment with all preparations.

From the general tenor of Dr. Hempel's report, one might infer that while the middle and higher potencies produce symptoms only in very exceptional cases, massive doses of the strongest preparations act always. The records of the Austrian provings hardly warrant such an assumption. Of the thirty-one experiments made with tincture of Aconite, six were not followed by any trustworthy symptoms. Of the thirty-six made with large doses of tincture of Thuja, seven were without result. Of fourteen made with large doses of crude Colocynth, one was without result. Of the fifteen provers of Byronia, one at least took very large doses of the tincture without observing any symptoms. Several who took large doses of crude Natrum muriaticum were insensible to its action. It is obvious, then, that the development of symptoms is not of necessity copious and satisfactory in the ratio of the dose. The Austrian records abound in editorial remarks, intimating that provers have

defeated their object by taking *too large* doses, and by *repeating* the dose *too frequently*.

We have seen, then, that potency-provings may be trustworthy and that they are indispensable to a complete and exhaustive proving. It is to be observed that even the few Austrian potency-provings which we have, were not, generally, conducted in such a way as to demonstrate the full value of such experiments. In very many cases, only a few days were allowed for the development of symptoms before a larger dose was taken. In the few cases in which a different course was pursued, as in Wurmb's proving of Natrum muriaticum, clear and persistent and most valuable series of symptoms were obtained.

In order to obtain a perfect proving, the prover should ever bear in mind certain fallacies to which he is liable. These fallacies differ according to the dose which he is employing; they are equal for either extreme of dose. If he use the higher dilutions, he is in great danger of confounding "imaginary" symptoms with real *subjective drug* symptoms. If he employ massive doses, he is in equal danger of including in his record *chemical* and *mechanical* symptoms. Either error is a source of confusion and uncertainty, but he is greatly mistaken who thinks to avoid introducing confusion and uncertainty into the Materia Medica by simply rejecting potency-provings. They can be avoided only by the constant analysis of the symptoms (whether produced with potencies or with large doses), by frequent repetitions of the provings on one's self and others, and by clinical verifications of the symptoms. The Austrian records give an example of the latter fallacy. The symptoms are so intermingled with chemical and revolutionary effects, that one cannot use too great caution in basing a prescription upon them.

In conclusion, we may assume the following points to be established by induction and by direct experience:

In order to obtain an exhaustive proving:

1. We must prove the drug *both* in dilutions and in massive doses.

2. The proving should be *commenced* with *dilutions*; and *high dilutions* should be employed until satisfactory evidence is obtained that the prover is not susceptible to their action. We thus obtain *one* of the unknown quantities of our problem, viz., the measure of the susceptibility of the prover.

3. Where a keen susceptibility is found to exist, the greatest care must be exercised to avoid *blunting* or perverting it. With this view, repeated experiments should be made at long intervals, with *high potencies*, until no new varieties of symptoms are evoked. Then, after a long period of non-medication, the prover should take lower potencies and then small doses of the crude substance repeated at intervals, and finally after another long period of repose, *large* doses of crude substance. A thorough proving after this fashion may require years for its completion—but it will have an advantage over most of our recent provings, in the fact that it will *be thorough*, and that it will be of permanent and certain use to the practitioner.

4. In proving with dilutions, as well as with massive doses, a long period of time should be occupied in testing each preparation, in order that the full effect may be seen in the production of dyscrasias, etc.

5. The greatest care should be exercised in *verifying* symptoms by repeated experiments, in order that "imaginary" symptoms on the one hand and chemical and mechanical symptoms on the other may be excluded. The fashion, which has become very prevalent of late, of including in the pathogenesis every sensation which occurs during the proving, without distinction or verification—and which may be called the *Pre-Raphaelite* method of proving—cannot be too strongly rebuked.

ALTERNATION OF REMEDIES. No. 1.

The following remarks upon the interesting, but, in our judgment, unsound paper on this subject by Dr. J. R. Coxe, jr., published in the February number of the current volume of the American Homœopathic Review,[1] may serve at least, it is hoped, to call attention to this very important approach to Polypharmacy in the customary method of prescribing, and, perhaps, to stimulate a discussion of its necessity and propriety.

In admitting that the alternation of remedies is incompatible with the theory of a true homœopathic prescription, our colleague has granted all that the opponents of alternation could reasonably ask. That our practice has not yet come up to our theory, and that we are not yet fully possessed of the means and knowledge to bring it up to that degree of perfection, and that therefore some prescribers may feel constrained *sometimes* to alternate remedies, as it were *under protest*, may not be denied. But to urge that *because* our practice lags thus behind our theory, we are to rest satisfied with it, nay, to *defend* it and to *conform our theory to it*, is a position that would be fatal to all progress, and which we cannot think of conceding. Nor is the appeal to *experience* in the sense in which our colleague uses it valid. He "has alternated remedies in a practice of twenty-five years, and his practice has been generally successful." We do not doubt this. But we believe his respected father practiced allopathy for a much longer period, and was a successful practitioner; would *this fact* be a valid argument in favor of *allopathy?*

[1] Vol. iii., 1863.

Alternation and *succession* of remedies are not generally understood to be identical processes. By alternation, we think, practitioners generally understand the prescription, at one and the same time, of two or more remedies to follow each other at short intervals, the symptoms of these remedies taken *altogether* being thought to cover the symptoms of the case more completely than those of either remedy *alone* would do. The prescription. is the result of one single examination of the patient and of one single comparison of the symptoms with the Materia Medica.

But when a succession of remedies is given, in either an acute or a chronic disease, the understanding is, that the first remedy, having been carefully selected, is allowed to exhaust its action alone, and *then* a collection of the symptoms the patient *then* presents is made, and the case is prescribed for *afresh*, almost as if it were a new case; and this process is repeated, each new prescription being the subject of a special study, until the case is cured. Such a process is equivalent to prescribing for a number of new and independent cases, and it is evidently not incompatible with the theory of a true homœopathic prescription.

The very thoughtful article by Dr. Payne, to which reference is made by Dr. Coxe, contains nothing incompatible with these views. He instances cases in which a remedy which had covered all the symptoms acts well for a time, then ceases to act, and leaves the case still uncured. He advises then a fresh study of the case, and especially a study of the previous history of the patient, and the prescription of a suitable antipsoric or antisyphilitic—in fact, a strictly homœopathic prescription *de novo*. Now, in many such cases, acute though they appeared at the outset, if the prescriber had studied thus carefully at the very beginning, not only the present symptoms, but also the previous history of the patient, a single antipsoric prescribed at first might have sufficed for the case. For although chronic diseases may be incurable except by an antipsoric, antisyphilitic or anti-

sycotic, yet these remedies may also be required in the very beginning of diseases that appear to be most *acute*. I can certainly testify, from repeated observation, to the efficacy of Sulphur[30] in most violent puerperal peritonitis, that remedy being all that was required to bring the cases to a speedy and favorable issue. This calls to mind Hahnemann's admonition—*Organon*, § 257. * * * The physician "should never lose sight of this great truth, that of all known remedies there is but one that merits a preference before all others, viz.: that whose symptoms bear the closest resemblance to the totality of those which characterize the malady"

Be this as it may, Dr. Payne's advice clearly contemplates a new study of the case for every new prescription, and, if this plan be pursued, the treatment will be strictly homœopathic, even though a dozen remedies were required in succession before the case is cured.

Alternation, however, as generally understood and practiced, does not contemplate this new study of the case before a second remedy is administered, or before every change of remedies; and herein it fails to come up to the requirements of a true homœopathic prescription. And this is the kernel of the whole controversy.

Suppose a case of pleurisy. Bryonia covers all the symptoms. The patient improves for a time and then ceases to improve. The case is studied anew. Some symptoms are found to have vanished; some new ones have appeared. The physician studies the previous history of the patient more closely; he may or may not conclude that there is a psoric taint. At all events he perceives that Sulphur corresponds to the symptoms better than any other remedy. He prescribes Sulphur, therefore, just as though this were a new case that had just come under his treatment. This is an instance of *succession* of remedies. But each prescription is a strictly homœopathic one, and in every respect commendable.

Suppose another case. A patient has acute tonsilitis.

The physician collects his symptoms and prescribes "Belladonna, to be followed in four hours by Mercurius, and that again in four hours by Belladonna, and so on." These two steady nags, so familiar to all, in this relation, are sometimes enlivened by the company of Aconite and Coffea *(pro re nata)* where the prescriber has no scruples about driving "four in hand."

This is an example of *alternation* of remedies, and is objectionable on the following grounds: Suppose the first dose of Belladonna to have been a judicious prescription, and to have had an effect. How can the prescriber know beforehand that this effect will be just such as to cause the totality of the symptoms at the end of four hours to correspond exactly to Mercurius, and, what would be still more wonderful, how can he foresee that the effect of the Mercurius will be just such as to cause Belladonna to be better indicated than any other remedy at the end of another period of four hours? The second remedy *(a fortiori* a third or fourth!) in such cases must always be prescribed without a preliminary study of the actual condition of the patient as modified by the action of the first remedy. It cannot, therefore, be an exact homœopathic prescription.

Hahnemann has expressed himself fully on this subject in the *Organon*[1] as follows:

Sec. 272. "In no instance is it requisite to employ more than *one simple* medicine at a time."[2]

Sec. 169. "It may easily occur, on examining a disease for the first time, and also on selecting for the first time the remedy that is to combat it, that the totality of the symp-

[1] Fourth edition, British translation

[2] Experiments have been made by some Homœopathists in cases where, imagining that one part of the symptoms of a disease required one remedy, and that another remedy was more suitable to the other part, they have given both remedies at the same time, or *nearly so;* but I earnestly caution all my adherents against such a hazardous practice, which never will be necessary, though, in some instances, it may appear serviceable."

toms of the disease is found not to be sufficiently covered by the morbific symptoms of a single medicine, and that two remedies dispute the preference as to eligibility in the present instance, the one being homœopathic to one part of the disease, and the other still more so to another. It is, then, by no means advisable after using the preferable of the two remedies, to take the other *without examination*, because the medicine given as the inferior of the two, under the change of circumstances, may not be proper for the remaining symptoms; in which case, it follows, that a suitable homœopathic remedy for the new set of symptoms should be selected in its stead."

Sec. 167. "In short, if the application of an imperfect homœopathic remedy used, in the first instance, cause any accessory symptoms of some importance, the action of the first dose is not allowed to exhaust itself in acute diseases; the altered state of the patient is then to be examined and the remainder of the primitive symptoms to be joined to those which have been recently discovered, to form of the whole a new image of the disease."

Sec. 168. "A new remedy that is analogous may then be easily discovered among the medicines that are known, a single application of which will suffice, if not to destroy the disease entirely, at least to facilitate the cure in a great degree. If this new remedy is not sufficient to restore the health completely, then examine what yet remains of the diseased state and select the homœopathic remedy that is most suitable to the new image that results from it. In this manner, the physician must continue until he attains his object—that is to say, until he has fully restored the health of the patient."

The name and practice of Hahnemann, Bœnninghausen and Hering have been adduced in support of *alternation*. Hahnemann's views on the subject we have already quoted from the *Organon*. The case to which Dr. Coxe refers, as cured by Hahnemann by "the alternate use of Bryonia tinct-

ure and Pulsatilla," has escaped our notice, and we should esteem it a favor if Dr. Coxe would inform us whereabouts in homœopathic literature it is recorded.

Hahnemann published very few cases. In vol. ii. of the *Materia Medica Pura*, third edition, 1838, he relates *two cases*, *one* of which was cured by a single dose of Bryonia tincture, and the *other* by a single dose of Pulsatilla. May Dr. Coxe have, possibly, referred inaccurately to *these* cases and, by a fault of memory, blended them ?

It is often stated, however, that Hahnemann used Bryonia and Rhus toxicodendron alternately in his very successful treatment of the typhus or hospital fever which prevailed so extensively in northern Germany, in 1813–14, after the French invasion. A reference to his article on the "Treatment of the Typhus or Hospital Fever at present prevailing," originally published in the *Allgem. Anzeig. der Deutschen*, number six, 1814, and translated by Dr. Dudgeon among "Hahnemann's Lesser Writings,"[1] will show how erroneous this statement is.

He says, "This fever has two principal stages," of which he gives the distinguishing symptoms; the first stage being characterized by "*pains and consciousness*," the second stage by "*delirium* and *mania*."

"In the first stage," he continues, "two vegetable substances are of use and generally quite remove the disease at the commencement—the Bryonia alba and the Rhus toxicodendron."

He proceeds to give the special indications for each of these remedies. "If the patient complains of dizziness, *shooting* (or jerking-tearing) *pains* in the head, throat, chest, or abdomen, etc., which are *felt particularly on moving the part*—in addition to the other symptoms, the hæmorrhages, the vomiting, the heat, the thirst, the nocturnal restlessness, etc., we give a single drop of Bryonia 12. Improvement takes place in the course of four and twenty hours, and *as long as*

[1] P. 631 *et seq.*, American edition.

the improvement goes on, we give him no other medicine, nor even repeat the same dose.

"If now the amendment produced by the single dose of Bryonia goes off in the course of two, three or four days, that is to say, if the patient *then* complains of *shooting pains in one or other parts of the body whilst the part is at rest;* if the prostration and anorexia are greater, if there is harassing cough or such a debility of certain parts as to threaten paralysis, we give a single drop of Rhus toxicodendron[12] and no more nor any other medicine so long as the improvement is manifest and continued. * * Or, if the symptoms I have just described occur at the very *commencement* of the attack, we give *at its very commencement* a drop of Rhus toxicodendron[12]." * * * *

"The whole disease will generally be removed by a single dose of the first or of the second medicine (according as the one or the other is indicated, without the addition of any other).

"If, notwithstanding, the disease should pass into the 'second stage' of delirium and mania, then Hyoscyamus niger meets all the indications of the case."

These detailed directions for the treatment of an epidemic disease give a perfect picture of a true homœopathic treatment. They state, in substance, that the *generic* symptoms of the disease are always accompanied in the commencement by one or the other of two groups of *characteristic* symptoms. Accordingly, as one or the other of these groups is present, Bryonia or Rhus is to be given. When the consequent amelioration ceases, then the case is to be examined anew, and *according to the characteristics then found to present themselves*, a *new remedy* is to be selected, and thus the practitioner is to go on until the case is cured. But *alternation* is nowhere advised or allowed.

Hahnemann's brief and very general hints relating to the treatment of croup are of the same general character. In the introduction to Spongia,[1] he says: "Homœopathy has

[1] *Materia Medica Pura*, vol. vi., second edition, Dresden, p. 199.

found, in symptom 145 and in other symptoms, the most remarkable applicability of Spongia to that terrible acute disease—*membranous croup*—provided, however, the *local inflammation* has first been diminished or subdued by a very small dose of Aconite. The additional use of a small dose of Hepar sulphuris will rarely be found necessary." Nothing is here said or in any way intimated concerning alternation.

In giving *general* directions beforehand for the treatment of this disease, which usually presents a very uniform group of symptoms, he shows that the symptoms, while the local inflammation is still raging, call for a very small dose of Aconite; that when this has acted, it will generally be found that the local inflammation is subdued, leaving a group of symptoms which correspond best to the symptoms of Spongia; that sometimes there may be such a modification as to call *instead* or *subsequently* for Hepar sulphuris.

When a physician has it in his power to visit his cases frequently, and to watch their course, he has no excuse for deviating from the methods of a true homœopathic prescription. If it prove necessary to give a succession of remedies, he is bound to make each successive prescription the subject of a special study, both of the symptoms of the patient and of the provings in the Materia Medica, and to select on each occasion the remedy which corresponds best to the symptoms of the case.

But where, as sometimes happens, the physician is unable to see the case at all, or where he is giving general directions for the treatment of a case which it is supposed may, at some future time, occur, this accuracy is impossible. His duty is then to describe to his client the symptoms which will probably be present in each stage of the disease, if it appear in the ordinary form and follow the ordinary course; to point out the indications for the appropriate remedy in each stage; and to instruct his client to give only one medicine at a time at each stage, and never to repeat the dose, or to give any other remedy so long as amelioration is

manifest. This is the best the physician can do when treating disease under the disadvantage of never seeing the patient. This is the method adopted by Hahnemann with reference to hospital typhus. It is the method of Dr. Bœnninghausen in treating croup.

It is well known that Dr. Bœnninghausen does not visit his cases, but prescribes chiefly in his offices. He knows that the first symptoms of croup are almost uniformly combined with a local inflammation which, along with all the other symptoms, forms a group to which Aconite corresponds better than any other remedy. His first powder, therefore, is Aconite. And his instructions are explicit. The second powder is never to be given until the amelioration, consequent on the first, shall have ceased, and so with the remaining powders. The recipients of "croup powders" are especially and carefully instructed under what circumstances alone it is proper to give any or all of the subsequent powders. This is as near an approach to a new study for each change of remedy as is possible when the physician does not see the patient.

It does not really amount in most cases even to a *succession*, much less an *alternation* of remedies, for in a large majority of cases the Aconite alone cures the patient.

That Dr. Bœnninghausen does not regard this method as an "alternation of remedies" is shown in vol. ii., p. 561, of the American Homœopathic Review.

Indeed, in *theory* and in *practice* Dr. Bœnninghausen is as decidedly opposed to alternation as we have shown that Hahnemann was.

We believe we could say that Dr. Hering's views are similar to those we have expressed, but we prefer to await a statement from his own vigorous pen of those arguments, both theoretical and practical, which none could give so well as he.

In conclusion, we hope it is hardly necessary to add, that inasmuch as offensive language and odious epithets lend no

strength to argument nor sweetness to persuasion, we entirely agree with our colleague in his closing paragraph. Nay! *for that very reason*, he will allow us to say, we could almost wish his own expressions had been a very little less emphatic.

[1] ALTERNATION OF REMEDIES. No. 2.

When, in the February and April numbers of this Review,[2] vol. iii., 1863, we published Dr. J. R. Coxe's article on Alternation, and our own modest objections to what we regarded as unsound argument and inaccurate statement on the part of Dr. Coxe, we had no idea that we were entering upon the discussion of a question, about the *terms* of which there could be any chance for a misunderstanding. Subsequent publications, from various quarters, have shown that all practitioners do not understand the same thing by the word *alternation*, as applied to the mode of prescribing homœopathic remedies. We think we shall be able to show that the practice which some writers defend, under the name of alternation, is not properly called by that name. But, before entering upon the subject itself, we desire to make, once for all, two statements of general principles by which we are governed not only in the treatment of this question but of all questions that concern the practice of medicine; premising that we utterly disclaim any disposition to dogmatize or to speak as with *authority* on any question of medical practice. If the earnestness of conviction should betray us into too positive warmth of language, this is our misfortune; and none can disapprove it more decidedly than we regret it.

1. The business of the physician is to *cure* his patient; this is the great practical object of his labor. We believe that nature is not always restricted to a single path—and that while some cases are so severe that there is but one

[1] *American Homœopathic Review*, 1865. [2] *Ibid.* 1863.

way in which it is possible to cure them, there are often *several* ways in which it may be possible to effect the cure of some other case; each way having more or less of inconvenience attaching to it and of detriment resulting from it, in proportion as it differs, more or less widely, from the *best* way. Thus, we believe that Allopathy, with her heroic *antiphlogistics*, her *revulsives* (borrowed from the "circumlocution office"), and, still more frequently, with her blind and blundering misuse of *specifics*, does sometimes "*cure*" her cases; but this is not the *kind* of cure that should satisfy the physician's desires. It is neither *safe* (inasmuch as it is debilitating in itself and is likely to involve damaging sequelæ), nor *rapid*, nor *pleasant*. Thus, likewise, we know that Homœopathists who mix medicines, whether in the tumbler or in the patient's stomach (conglomerators or alternators), do often "cure" their patients. These cures, if our observations be correct, are neither so *rapid* nor so *pleasant* (free from sequelæ) as cures might be; and we are confident that by these methods not nearly so large a proportion of the sick may be cured as by adherence to the simple, single remedy. Still, we admit that when a physician has cured a patient, by whatever method, he has, *in so far as that patient is concerned*, done the chief part of his duty. Though his method were not the best of known methods, he is not to be blamed without qualification.

2. But, although, from the ethical stand-point, we may concede that:

> "'T is better to have" *erred and cured*
> "Than never to have" *cured* "at all,"

we think there is abundant reason for cautioning the practitioner to beware of confounding the misfortune of the *error*, with the good-luck of the *cure*.

As one to whom have been confided the interests of that individual patient, he may justly rejoice in the cure; but do his functions cease here? By no means. He is a man of science, to whose care are to be intrusted, every day of his

active professional life, case after case of disease which he is to bend his energies to cure. What is it to be a man of science? It is to be one whose mind is stored with an array of facts carefully observed by others as well as by himself, and methodically arranged, in such wise that principles have been and may be correctly inferred from them—principles by virtue of which *new* observations may be arranged along with these facts—principles by the aid of which the facts which will result from operations observed to be in progress, or intentionally put into activity, may be accurately predicted, and, conversely, may be produced at pleasure.

Such is the scientific physician. Let us note the difference between the action of *his* mind and that of the *patient*, after a cure of the latter has been accomplished by the former. The patient says to himself, "I was sick and now I am well. I will pay my doctor and then, to my work again!" This is all his sickness is to the patient.

The soliloquy of the doctor will depend very much upon the nature of the mental process by which he arrived at the mode of treatment that cured the patient. It may be:

1. "How can I ever be thankful enough for the lucky accident that made me give him Ledum! May I be as fortunate when I guess again!" or,

2. "When I gave Bryonia alone and Sepia alone, though each seemed to correspond pretty well, still the patient did not get much better. But when the happy thought of alternating them occurred to me, and I did it, she recovered. From this I shall learn that two remedies, each of which corresponds to part of a case, may cure the case if given in alternation, when neither, if given singly, would cure it;" or,

3. "A year ago I should have given, for such an angina as this, Belladonna and Mercurius in alternation, as Rummel recommends, and the patient would probably have gotten well in two or three days and I should have been satisfied; but since I have studied Lachesis, I find that remedy covers

the whole case much better than Belladonna and Mercurius would do; and lo! he is well in twelve hours. From this, I learn to shun, more earnestly than ever, those expedients which, like alternation, are borrowed from the polypharmacy of the Old School and which, being *opposed* to sound principle, must be inferior in their results to some better way which *accords* with sound principles and which way I shall be most likely to find out if I make my practice follow principle. This case, then, gives me fresh zeal in my study of Materia Medica;" or,

4. "This case cost me much study. I saw that the symptoms of the abdomen and digestive canal as well as those of the lumbar region and of the hip and thigh and the general conditions of aggravation and amelioration corresponded admirably to Colocynth, but then, the patient had, in addition, an enlarged ovary (from which, possibly in some way, the other symptoms sprang), and I have never heard of Colocynth in connection with enlarged ovary. I might, therefore, have been tempted to alternate Colocynth with some remedy which is known to have cured and perhaps produced enlarged ovary, such as Lachesis, Apis, Graphites, Lycopodium, Staphysagria, etc., but for my aversion to disregard what seems to me the well-established principle: that maladies are not local affections, but general, pervading the entire organism—that the individual man is not an aggregation of independent monads, each of which may be ill or get well 'on its own hook,' without its neighbor being thereby jostled, and against which may be discharged a corresponding load of medicinal monads each of which will find its own particular target without hitting any other—not this, but an *individual being*, whose functions and tissues are so intimately connected, that, 'if one member suffers, all the members suffer;' and conversely, if we get the key to the malady by finding the characteristic symptoms which will point us to the true remedy, we shall cure the *entire* suffering, to whatever member we may address ourselves. In this

faith, regarding the characteristic symptoms as calling unmistakably for Colocynth (there was nothing characteristic of any remedy in the ovarian symptoms — the ovary was enlarged, that was all), I give that remedy. And now, while in a few days the other symptoms which plainly called for Colocynth disappeared (permanently, as the event proved), in the space of two months the ovarian tumor had likewise disappeared, and the patient, who had been confined for six years to her room and couch, could now walk, drive, and go about the house as well as ever.[1] From this I learn the *unity* of disease, and by this I am strengthened in my belief that adherence to well-settled principles will, in the end, carry one farther and faster, even over dark and uncertain ways, than any make-shift and irrational expedient would be likely to do;" or,

5. "In this case of dysmenorrhœa the symptoms of the head and eyes are very characteristic of Cyclamen. But, surely, the menstrual symptoms are too important to be overlooked in this case, and the remedy must correspond to these symptoms in order to cover the case. I looked for the symptoms of Cyclamen on the female sexual organs, and lo! none are recorded in the *Materia Medica Pura.* It seems that there was not a woman among the early provers of Cyclamen. What was to be done? The menstrual symptoms corresponded pretty well with those of Pulsatilla, though the head and eye symptoms did not correspond. Should I alternate Cyclamen and Pulsatilla, which jointly cover the case? I should have high authority for such a course! But, I reflect, that the *same kind* of a human organism which, under some disease-producing influence, experiences, at one and the same time, the amenorrhœa and the head and eye symptoms of my patients — the *same kind of human organism,* I say, experienced also, when proving Cyclamen, head and eye symptoms exactly like those of my patient. Is it not fair to presume that, if this disease-

[1] See "Observations on Colocynth."

producing cause and the specific properties of Cyclamen are so nearly alike as to produce identical symptoms in the *head* and *eyes*, they would have produced identical symptoms likewise in the female sexual organs, had the prover of Cyclamen been a woman? So strongly did probabilities seem to me to favor this presumption, that regarding alternation as an unsound and irrational expedient, I was about to take the risk of giving Cyclamen alone on the strength of this anticipated result of some future proving by a woman, when, chancing to meet with the new Austrian proving of Cyclamen, I found that provings by women are there recorded which confirmed my presumption in every particular. The case recovering speedily under Cyclamen, I learn from it that in many cases we are tempted to alternate because we cannot cover every feature of the case with either of the remedies which we think of alternating. But, in some of these cases, the symptoms which *are really characteristic* are fully met by *one* of these remedies, and there is ground for assuming, as clinical records show, that subsequent and more extended provings will demonstrate to us that this remedy, if fully proved, would really cover the entire case, *characteristics* and all This experience fortifies me against a temptation to alternate, and leads me to rely more confidently on the indications furnished by *characteristic symptoms*."

If we review these cases and analyze the operations of the physician's mind, we shall see that, in number one, he confesses that he has been the "accident of an accident," and he invokes his "good luck" to stand him in stead again. He does not exercise his reasoning faculties at all. Let him pass.

In the other cases, there is, besides a thanksgiving for success, an effort of the mind to arrange the facts which the case presents in order, along with some other facts already stored there, and to infer from the aggregate store of facts some principle or plan of action which may be profitably

brought to bear on some future case. It is thus and *only* thus that "*experience teaches.*" For, since no two cases are ever met with that are in every respect precisely alike, the experience acquired in treating one case can never be available in treating another, except through the intermediate application of the reasoning powers. We hear and observe facts, collect and arrange them, analyze and reflect upon them, induce principles from them, and prepare ourselves to make practical application of these principles when a new case shall call for it.

Now, what is this but theorizing? When we analyze a case in such a way as to suggest to our minds its proper mode or course of action, or to infer from it any principle that might help us in a future case, we form a "theory." We cannot *think*, indeed, about collecting facts without *theorizing*.

Yet the defenders of alternation invite us simply "to look and see," to "establish facts. These once fixed, any theory which will perfectly account for them is good." These are Dr. Hawley's words.[1] And the *London Homœopathic Review*, vol. ix., p. 432, quotes Dr. Hawley's words approvingly: "The homœopathic system of medicine," * * * says Dr. Hawley, "bases itself, not on theories but on facts as they are observed in the world of man. It frees its disciples from all dogmas and simply asks them to look and see." Well, being thus invited, we "look and see." What do we see? Why we see Dr. Hawley giving Bryonia and Sepia in alternation in one chronic case and Arsenic and China in another case, and curing both in a way which satisfies him well. This is what *we* see. But what does Dr. Hawley see when he looks at these same facts? Why he sees something which proves to him that "for him, the use of remedies in alternation is better than the use of a single remedy." And the *London Review* sees in the *same* facts something "which proved to him (Dr. Hawley) that the

[1] *American Homœopathic Review*, vol., v., p. 338.

alternation of medicines is not only admissible, but that cases now and then occur which CAN ONLY BE CURED BY SUCH ALTERNATION."

Is not this "*theorizing*" pretty strongly and on a rather slender basis? Our friends warn us against "theory" and yet, from two *facts*, Dr. Hawley concludes that, "for him, the use of remedies in alternation is better than the use of a single remedy." From the same two facts the *London Review* makes Dr. Hawley conclude that "cases now and then occur, which CAN ONLY BE CURED BY SUCH ALTERNATION." This is a broad generalization on a very narrow foundation. But Dr. Hawley admonishes us that "we have not yet any such *collection of facts as will warrant any generalization.*"

For ourselves, we should not feel justified in drawing any such conclusions as these from Dr. Hawley's cases or from any of the cases already narrated, cured by alternation. When adduced in evidence, we accept them as facts and give them what we consider to be their full value. They prove to us that cases may be cured by alternated remedies, but they prove no more than that. They prove that two remedies in alternation cured a case which neither of those two remedies singly had cured. Beyond this they prove nothing. Assuredly they do not prove that a physician could not have cured each case more quickly with some other remedy, given singly. How could such evidence be held by us to justify such conclusions as Dr. Hawley and his reviewer draw from it, when our daily experience furnishes case after case which had been treated ineffectually by physicians who *always alternate* and which yields promptly to the single remedy. Evidence of this kind whether FOR or AGAINST alternation, will not settle this question.

These remarks, extended as they are, have been made for the purpose of showing that clinical experience is available as a means of improvement in medical practice only in so far as it is analyzed, thought about, and, in fact, "*theorized*"

about; that those who deprecate dogmatism and would put off the formation of generalizations, do, themselves, dogmatize and theorize, and from the nature of the case they *must* do so in the act of reasoning about the evidence they bring forward.

If this be inevitable, then, it must be allowed *us* likewise to theorize and in what we have to say about alternation we shall hold ourselves justified in basing our argument on generalizations from a multitude of collated facts. We hold that the argument from theory is in order. And regarding a SCIENCE as being a connected and independent series of generalizations based on an analysis of methodically arranged and collated facts, we require the advocates of alternation to rest their cause, as we do our opposition to alternation, on such a generalization. Failing to do this, they have no claim for their method as a part of the SCIENCE of Therapeutics. Failing this, the facts which seem to justify alternation can be used in no other way than in the blind, empirical way of literal *imitation*, in which accuracy and certainty are quite out of the question. But our whole object in study and labor, beyond the direct need of our patients, is to complete the structure of our science, such as we have defined a Science to be, a means of attaining accuracy and certainty.

Some of the advocates of alternation have failed to perceive the necessity of raising their procedures to the level of a scientific method. They still rest on the rude empirical ground of unmethodized experience. Their argument is: "I have alternated remedies which, singly, had failed, and I cured. Henceforth I shall alternate." Or, as the *London Review* varies the argument (9, 432), "The practice of alternation of remedies is one so widely adopted that it would appear to have the sanction of very extended practical experience. * * * Experience has proved abundantly that the alternation of remedies increases the rapidity of the cure," etc. The same argument was used by Dr. Coxe,[1] who claims to have alternated for twenty-five

[1] *American Homœopathic Review*, iii., 59.

years, and to have been successful. As we have before remarked, the same argument may be used by Allopathists, and by the advocates of every form of practice, whether pure, mixed, or wholly vicious. The same argument may be, and is, advanced by those who oppose alternation.

Others, however, have seen the necessity of basing their advocacy of alternation on some general principles, among them particularly Dr. Drysdale[1] and Dr. Coe,[2] and to these statements of principles we wish to devote some attention.

But, first, let us have a clear understanding of what is properly meant by alternation of remedies.

As we have stated, the term is used in different senses.

1. Dr. J. R. Coxe (loc. cit.) seemed to think that the opponents of alternation contend that each case of sickness should be treated with one single remedy, and that, if during an illness a change in the symptoms should compel a change in the remedy, this, by whatever name it be called, is, in *fact, alternation.* He scouts the idea of any real distinction between *alternation* and *succession* of remedies. Well, if this be all that alternation means, we have no reason to oppose it. But, what is a case of disease in this sense? Does it comprise all that may ail a man from the time that he takes to his bed to the time that he goes to his work again? Suppose a patient sick of dysentery, and recovering finely under the single remedy Mercurius corrosivus. When just convalescent he is seized with rheumatism, and requires Rhus toxicodendron. Is it "alternating" to give it to him? And then, suppose him safely over rheumatism; but just before he goes to work again, he has a return of dysentery, requiring Mercurius corrosivus. Is it "alternating" to give it, if the symptoms require it? And if, when cured of this relapse, he gets the measles and requires Euphrasia, is it "alternating" again to give him this remedy? Why, according to this definition, if you call this all one sickness,

[1] *Annals of British Homœopathic Society.*

[2] *The American Homœopathic Review*, vol. v., April, 1865.

it *has* been a case of alternation. If you call it four sicknesses, perhaps not. To the patient, certainly, it is "all one," whatever you call it! Now, suppose the symptoms in a case to have changed just as decidedly as in the case supposed above, but yet not so definitely as to induce you to give to the changes these nosological names of *dysentery*, *rheumatism* and *measles*. The medicines, Mercurius, Rhus and Euphrasia will have been just as clearly indicated by these successive changes, and just as imperatively required as though the patient were regarded as having had four successive diseases. Is it "alternation" to give these remedies successively just as they become clearly indicated? We think it is not, but some say it *is*. We are sure it is sound practice.

2. Dr. Quin, of London,[1] calls the method of prescribing successive remedies in a case as the changes of symptoms may require them, "*alternation a posteriori*," and sanctions and defends it. But he reprobates, under the name of "*alternation a priori*," the practice which, it seems to us, is the only one which can properly be called "alternation," viz.:

"Prescribing at the very outset of the treatment—at the very first visit and also, not unfrequently, almost at every subsequent visit, two or more medicines, to be alternated every quarter, half hour, or every two, three, or four hours. It is difficult to believe," he continues, "that such practitioners are in the habit of carefully considering the cases under treatment, or have well studied their Materia Medica, or to divest one's self of the idea that they resort to such slipshod practice in the hope that if one of the medicines does not hit off the complaint, some one of the others may. One meets with instructions for similar alternation laid down in popular books on Homœopathy, showing that this *a priori* style of alternating remedies is, with certain practitioners, more a rule than an exception."

3. Dr. Drysdale defines *alternation* to be "*the giving a second medicine while the sphere of action of the first is still unexhausted*." But Dr. Drysdale, while advocating alternation, *in this sense*, in certain specified cases, does not propose to give his second medicine except after a careful

[1] *Annual Address, Annals of British Homœopathic Society.*

re-examination of the symptoms of the patient, and a comparison of them with the Materia Medica. It is clear, then, that alternation as *he* defines and defends it, is not the same thing as the alternation *a priori* which Dr. Quin reprobates, and to which *we* would restrict the term. Dr. Drysdale's definition seems to us very vague and impracticable. How are we to know when the sphere of action of the medicine is exhausted? By inferences from the proving? But we have reason to believe that the speed at which vital processes go on in sickness may be very different from that during a physiological proving. Reduced to a practical rule, Dr. Drysdale's reasoning would amount to this: If, on our second visit, symptoms shall be found to have arisen which seem to call for the second medicine, we should suspend the first and give the second, and then, if, at the third visit, the symptoms be found to have changed again, so as to call again for the first medicine, we should give it. But this, he says, is "*alternation*" With certain qualifications we agree to his *rule* of practice, but we object to the *name* he gives it. And here names *are* important. It is of great consequence to avoid giving to two radically different procedures one and the same name.

Now, what do *we* mean by "*alternation?*" Let us give a practical answer. We have before us a prescription label filled up by Dr John Doe It reads thus:

"No. 1, Acon[3]; No. 2, Bellad.[6]; of each, twelve powders. Take the powders alternately as numbered, every four hours."

We have another paper containing directions prepared by Dr. Busy, for a chronic patient:

"Take as follows: Mercurius hydriod., first decimal trituration, every night for seven nights;

"Then Sulphur[3], every night for seven nights;

"Then Collinsonia[1], every night for seven nights;

"Then Podophyllum pelt.[2], every night for seven nights;

"Then Ignatia[3] and China[1], alternately every night and morning, for seven days;

"Then Aurum met.[1], every night for seven nights;

"Then Pulsatilla[1], every night for seven nights;

"Then Phytolacca dec.[1], every night for seven nights."

We object to these, and to all kindred procedures, that they rest on hypotheses which are not warranted by the present possibilities of science. Dr. John Doe's first prescription of Aconite may be all right; the symptoms probably call for it. But the physician cannot know in advance that, in four hours, the symptoms will have so changed that, if he were present, he would perceive Belladonna to be indicated; and that, in four hours later, Aconite will be indicated again; and that the symptoms will go on, oscillating between these two series of indications, each oscillation consuming just four hours.

And "Dr. Busy," by what wonderful gift could he foresee that seven days of Mercurius hydrargyrum would bring his patient to a state requiring Sulphur, and seven days of Sulphur to a state requiring Collinsonia (whatever that state may be), and seven days of Collinsonia to a state calling for Podophyllum (and for just seven days of it too), and seven days of Podophyllum to a state of complex misery that calls for China and Ignatia in alternation (for just seven days likewise) and so on to the end of this long chapter!

"Oh, wad some power that giftie gie *us!*"

These instances, and the comments upon them, comprise our definition of "alternation," and our objection to it. It is a practice very prevalent in the United States and in England. It is exceptional in Germany, France, Spain and Italy.

The principles which govern the selection and administration of homœopathic remedies are very simple.

The great law, SIMILIA SIMILIBUS CURANTUR, teaches us to select a remedy the characteristic pathogenetic symptoms of which are very similar to those of the patient. This is a grand generalization, supported by a multitude of facts. We accept it. It takes no heed of *names* of diseases, nor of pathological theories of the seat and origin of diseases. Giving a broad and liberal signification to the word "symptom," so as to include everything abnormal about the patient, whether it be historical or actual, this law pays regard to the symptoms alone. It requires that the symptoms shall be collected and compared with the Materia Medica *every time a prescription is made*, and that the drug that has produced symptoms most similar to those of the patient shall be chosen and given. This is a true homœopathic prescription. No matter how often during the sickness of a patient this process be repeated; no matter how many remedies be given in succession; no matter if the first remedy be recurred to after the second and the second after the first—if each prescription have been the fruit of a special collection of symptoms and comparison of them with the Materia Medica—call it "*alternation*," or by whatever other wrong name you please, it is a sound and defensible homœopathic prescription, such as Hahnemann taught and practiced, and his followers adopted.

But the physician sometimes leaves a second remedy to be substituted for the first under certain specified contingencies. Is this alternation? By no means. In so doing, he makes the attendant his deputy, and describes the series of symptoms which, in his judgment, will be an indication that another remedy is required. He is merely instructing and empowering another person to make, in his stead, the study and selection of a remedy which should precede and be the basis of every new prescription.

And wherever, in his writings, Hahnemann has seemed to authorize or sanction alternation, his directions have been of this character. He has mentioned that a certain group

of remedies is likely to be indicated in a certain disease, and although he has sometimes used the *word* "alternate" *(abwechseln)*, yet, in every case, he has specified the particular symptoms, or groups of symptoms, which would specially indicate and authorize the preference to be given to one or another remedy of these groups.

In a previous paper[1] we showed this to be true of Hahnemann's directions for the use of Spongia, Hepar and Aconite in croup, and also for Bryonia and Rhus in typhoid fever. The same is true of his directions touching cholera.

The *London Homœopathic Review*[2] says: "Those who combat 'Alternation' on the ground that it is opposed to the *practice* of Hahnemann, fall into a grave error. In the introduction to Belladonna in his *Materia Medica Pura*, he advises the *alternation* of Aconite and Coffea cruda in purple rash, in these words: 'Aconite and Coffea should be alternately given every twelve, sixteen or twenty-four hours, in proportion as one or other remedy is indicated.'"

It is true that the above words *are* contained in the introduction to Belladonna, but they are not *all* the words contained in the sentence from which they are quoted. This sentence, complete, is as follows: "There (in the purple rash) Belladonna, naturally, does no good, and the other, common, charlatan treatment must also allow the most of the patients to die of it, whereas they might all be cured by the alternate use of Aconite and of the tincture of Coffea cruda, the former for *the heat and the increasing restlessness and agonizing anxiety*, the latter for *over-excessive pains with a lachrymose humor;* the Aconite in the 30th dilution of the juice and the tincture of Coffea cruda in the 3d dilution, both in the dose of the smallest part of a drop, the one or the other every twelve, sixteen or twenty-four hours, *according as the one or the other is indicated.*" The two groups of symptoms which Hahnemann gives as indications, respect-

[1] *American Homœopathic Review*, April, 1863.

[2] Vol. ix., p. 432.

ively, for the one or the other of these remedies, are omitted by the *London Review*. They are the essence of the whole matter. Following this advice of Hahnemann, it would not be possible for the practitioner, on meeting a case of purple rash, to begin with a blind *a priori* alternation of Aconite and Coffea. On the contrary, as we understand Hahnemann's direction, he should examine his case to see whether it presented the groups of symptoms "heat, increasing restlessness and agonizing anxiety," in which case he would give Aconite; or whether the characteristics of the case were "over-excessive pains with a lachrymose humor," in which case he would give Coffea. Suppose at his next visit, twelve, sixteen or twenty-four hours after, he should find, as might well happen, that the group of symptoms first observed had disappeared and had given place to the other group, he would, for this reason, change his remedy. Or, if he lived at a distance from the patient, he might make the nurse his deputy, and, instructing this deputy as to the distinction between the groups of symptoms, might direct her (as Hahnemann has directed us) to make changes in the remedies when corresponding changes in the symptoms should call for them. In this way there would be no assumption and no *a priori* "alternation." The practice would be sound; fulfilling the conditions of a sound prescription, viz.: that each prescription be preceded by a fresh collection of the symptoms of the case, and comparison of them with the Materia Medica.

Is it suggested that Hahnemann meant to intimate that these groups of symptoms might co-exist in the patient at the same time and might make a "totality of symptoms" that would require these two remedies to cover it? But these groups are pathologically incompatible. No one conversant with the phenomena of sickness could conceive of a patient presenting, at one and the same time, "heat, increasing restlessness and agonizing anxiety," and likewise *over-sensibility to pain and a disposition to weep and despond.* No!

patients in a waxing fever often *swear;* but they seldom *pine* and *weep;* they feel general anguish but make light of specific pains. But when the fever has *waned*, there often succeeds it a stage of over-sensibility and of proneness to weep; and this succession may be repeated again and again, and we suppose Hahnemann recommended these remedies to correspond to this succession. We cannot comprehend his words in the sense attached to them by the *London Review*. In the signification in which *we* have understood them, they correspond precisely to all of his other directions, which have been quoted as favoring "alternation."

But, now, suppose it conceded that, while Hahnemann's practical deductions from scientific *principles* were opposed to "alternation," his *practice* sanctioned it. Is the argument from his *practice* all powerful against his *principles?* This reminds us of what the Chairman of the British Society calls Dr. Drysdale's Socratic irony "—you all object to alternation, and, yet, you all alternate." We have seen that Dr. Drysdale's definition of alternation is such as to cover almost every actual case of treatment and is different from that of every other writer. But, the argument from the universality of a practice, in favor of its propriety, is a dangerous one to play with. Place it in the mouth of a theologian and see how it reads: You all condemn *sin*, and yet, you are all *sinners!*"

Does the weight of this argument lie on the side of *sin* or on the side of the *condemnation* of sin! Does propriety necessarily follow from universality? Are the majority *always right*, just because they *are* the majority? If so, all hail, Allopathy!

The conditions of a true and defensible homœopathic prescription require, in our opinion, that the symptoms of the patient, at the time of prescribing, shall be collected and compared with the provings in the Materia Medica, and that the drug whose symptoms correspond most closely with those of the patient shall be selected. Perhaps no Homœo-

pathician would object to this statement. But let us see what it requires.

First. It requires that before every prescription, the symptoms of the patient shall be studied anew. In some way or other, whether it be done by the physician or by a provisional deputy specially instructed for the case (as we have explained that the nurse may be), more or less perfectly, this must be done. We have seen that in the ordinary method of alternation *(a priori)* this is not attempted to be done and cannot be done; it is not proposed to do it.

Second It requires that the aggregate of the symptoms presented by the patient be regarded as one malady, for which an analogue is to be found in the Materia Medica. We have no authority in science for arbitrarily dividing this aggregate of symptoms into groups, for each of which we are to find an analogue in the Materia Medica, and then giving these analogues, in combination, or in alternation. This requirement is perhaps the most difficult of all to fulfill. In collecting the symptoms, our utmost sharpness of insight and our deepest and most extensive learning in every department of physiological, psychological and pathological science will be tasked to construct, from the patient's history and from his present condition, a complete picture of the morbid phenomena which he presents, from which the physiological idiosyncrasies of his peculiar temperament and personality shall all have been eliminated, and in which his symptoms shall be duly arranged with regard to their mutual relations and dependencies. Then, furthermore, inasmuch as we cannot hope to find, in the proving of any drug, a duplicate symptom for every symptom of the patient, the rarest judgment and most extensive knowledge of semeiology are required to analyze the patient's symptoms and to detect those which are truly characteristic of that individual case, and for which an analogue must be found in the proving; and to set these characteristics apart from the other symptoms, the analogue of which it may be less impera-

tively necessary to discover. It is here, undoubtedly, that the greatest knowledge and ability are required of the physician and here that failures are most frequently made. It is, probably, from failures in this analysis of symptoms, that the supposed necessity of alternation most frequently arises.

Third. It requires that a drug shall be selected which has produced on the healthy subject, symptoms very similar to those of the patient. The substance given must have been proved in the *same form* (not necessarily the same *dose*) as that in which it is proposed to be given. If Hepar sulphuris calcareum correspond to the case, this requirement is, by no means, fulfilled if we give Sulphur and Calcarea carbonica combined or alternated, on the ground that these substances are the constituents of Hepar sulphuris calcareum. For, this involves the assumption that the Sulphur and the Calcarea carbonica have undergone no changes during the process which made, out of them, that third substance—Hepar sulphuris. No! Hepar sulphuris was proved *as such*. If we select it, for the reason that the symptoms which it produced correspond to those of the patient, then we must give Hepar sulphuris, the very substance that was proved. Otherwise we plunge into a sea of speculation and hypothesis and forfeit that certainty which it was the sole object of our science (as of every science) to attain. In like manner, if the Iodide of Mercury had been proved on the healthy subject and its symptoms were most similar to those of our patient, it would not be a compliance with the demands of our science should we give, instead of Iodide of Mercury the very substance that produced these symptoms, the Mercurius vivus and the tincture of Iodine, assuming that, because Iodide of Mercury is a compound of these two substances, therefore the conjoined or alternate action of the elements from whose union it sprang must be identical with its own. From instances like these, it is clearly to be seen that we may not, consistently with the principles of our

science, prescribe drugs in any other form or combination than that in which they were proved. If drugs had been proved in alternation, we might then with propriety, perhaps, prescribe them in alternation. Until this is done, the method is a hap-hazard, chance operation—successful, no doubt, at times, but in such a way that success could never be fore-seen or insured, nor could the experience of the practitioner in any way serve to establish or confirm any principle of medical science.

A homœopathic prescription, as we have defined it, is a deduction from a generalization, which has been established by induction from a multitude of instances. This is the law SIMILIA SIMILIBUS CURANTUR, in accordance with which the remedy is selected, under the three requirements that we have specified. So well established is this law of nature, that if we are so fortunate as to be able, in any given case of disease, to comply closely with those requirements, and particularly with the second, we may with certainty predict, and in confidence await, the favorable result of our prescriptions. Such certainty of foresight and such confidence it is our great object to attain, and nothing but a scientific method can afford them.

But we have shown that alternation, as *we* use the term, and as we have described the process, is incompatible with this scientific method. It does not meet the requirements of the law. It does not take the aggregate of the symptoms as the single basis of prescription. It does not give the remedy, single and simple, such as it was used in the *proving*. But it permits itself to act on two assumptions—that the aggregate of the symptoms may be arbitrarily divided and separately prescribed for; and that two or more drugs which have been proved independently of each other may be used conjointly as a sort of composite analogue to the aggregate of the symptoms; and all this with equally good and sure results. These two assumptions are not even *alleged* to be based on any collection of facts. Their introduction

deprives the proceeding of all claim to a strictly scientific process

The advocates and defenders of alternation are naturally divided into two classes. The one frankly disavow any pretension that alternation is a scientific deduction from a general principle, and defend it on the simple ground of experience. They have alternated successfully in a case or cases like the present, and therefore they do it again.

Now, obviously, with this class we cannot discuss the question on scientific grounds. There is no *reason* in their method, because there is in it no reference to general principle, to natural laws: it gives no means of foreseeing and providing for future results; it is the simplest form of literal empiricism. We can do nothing more than show, as has been done, its unreasonableness and the precarious and contradictory and disappointing character of its results. We can only point out how far it falls below the standard of action to which practitioners of medicine should hold themselves, and how unworthy, in our judgment, such methods are of reasoning and conscientious men.

The second class accept our definition of the requirements of a sound prescription, and our statement of the obligation of scientific men to abide by natural laws. But they claim that alternation does *not* necessarily, as we have maintained, contravene the requirements of such a prescription; and they claim that there are certain established principles in accordance with which we may, with scientific accuracy, determine when and how to alternate. This class defend alternation "on *principle*." As the former class comprises some of our most conscientious and estimable colleagues, so does the latter embrace many of the most gifted and learned of our school. Their opinions are worthy of the most earnest and respectful consideration.

We have previously stated that some who defend alternation on principle designate by that name methods which, we think, ought not to be so called, and which we do not

find fault with; as, for example, Dr. Coxe; likewise Dr. Drysdale, in so far as his definition is concerned. Their methods do not always, of necessity, conflict with the requirements of a sound prescription. There are still other procedures, called "alternation," but which are not always necessarily liable to the objections urged against "alternation," properly so called, as *we* have defined it *(a priori)*.

The occurrence of complications, and especially of traumatic complications, such as a burn of the hand, during the course of a pneumonia—a contused vulva simultaneously with a milk fever, are mentioned by Dr. Drysdale, as instances which may require alternation; as, for example, Urtica urens to the burned hand, while Phosphorus is being given internally for the pneumonia, and Arnica to the vulva, while Aconite is given for the milk fever.

Now, of such cases as these it might perhaps be properly said, that they do not come under the scope of our inquiry, inasmuch as the burn and the contusion might be regarded as purely local, and not at all constitutional affections, and the respective applications might be viewed in the same light, and as not capable, when thus used, of affecting the general system, and of thus being, *in fact*, alternated with the remedy which the patient is taking internally. But, waiving this reply, we may say that the teaching of our own experience is, that, in such cases as these, the best way is to follow the one great rule which governs homœopathic prescription; after the occurrence of the complication, collect and scrutinize the patient's symptoms anew. If the burn shall have been so slight as to make no impression on the general system, to produce no modification in the symptoms of the previously existing pneumonia, then there is no reason for doubting that simple protection from the atmospheric air will be all that is needed in the way of treatment for the burn.

If, on the other hand, the burn have been of a serious character, so as to produce constitutional disturbance, it will

assuredly have modified the symptoms of the pneumonia, and we shall perhaps find, on taking the aggregate of the symptoms again, that some other remedy than phosphorus is now indicated to meet the new state of things resulting from the pneumonia and the shock of the burn combined, and that this new remedy will meet both troubles. For it must not be hastily assumed, as Dr. Drysdale seems inclined to intimate, that Urtica urens locally applied is always the best (or a good) remedy for *burns*. They are often (at least) best treated, as Bœnninghausen has shown, by an internal remedy. Singularly enough, it once chanced to myself to meet the complication which Dr. Drysdale here supposes. The burn was severe, and the effect of the shock so modified the symptoms of the pneumonia that the resulting aggregate of symptoms called unmistakably for Arsenicum, which was given with most gratifying results, as regards both pneumonia and burn. The same may be said of the application of Arnica to contused vulva. Indeed, there is too great haste among us to recur to topical applications.

The second "principle" upon which, according to Dr. Drysdale, "the practice of alternation rests," is "the maintaining the susceptibility." Dr. Drysdale says: "In disease we generally find that the susceptibility to the homœopathic remedy is present from the nature of the case, and our great object should be to manage the doses and repetitions so that it shall not be exhausted before the natural period essential for a cure. This unfortunately, however, not unfrequently happens, in spite of varying the dilutions or originally having chosen the best. In this case the plan has been adopted of interposing doses of another remedy as nearly as possible homœopathic to the case. This must, of course, be an antidote, but, as in the case of natural disease, it may tend to revive the susceptibility to the first remedy. * * * Without the use of occasional alternation and intermediate remedies, we should be almost deprived of the

use of a large class of serviceable remedies in chronic disease, such as Opium in constipation, Lobelia, Lactuca, etc., in asthma, Coffea and Aconite in neuralgia, etc."

This question of the propriety of alternating for the purpose of reviving the exhausted susceptibility may be treated in much the same way as the question of alternating to meet complications. When we have a case under treatment, we shall not be led to suspect an exhaustion of susceptibility, except by a change in the symptoms. Now, if the symptoms have changed, this very change furnishes us the basis for a new prescription. Why shall we not at once proceed, in accordance with the great rule of our art, to select a new remedy corresponding with these changed symptoms? Why should we prefer, to this certain method, the pathological hypothesis that the susceptibility to a remedy which we have assumed to be suited to the disease has become exhausted and needs a stimulant? This hypothesis may be correct, but can never be a certain basis for practice.

But let us take an example: Cases of dysentery not unfrequently occur in which Nux vomica or Mercurius seemed in the beginning to be very clearly indicated. The patient improves for a few days under the use of one of these remedies, and then ceases to improve. On a careful scrutiny of the case it appears that the prominent *characteristic* symptoms (most of them *subjective*, of course) have disappeared or become obscure. Even the evacuations have ceased to be characteristic of any remedy. The entire system is dull, languid, unimpressible. In such a case as this, we are told that the susceptibility of the patient has become exhausted; that a few doses of Opium will arouse it so that it will again respond to the appropriate remedy, and will be cured.

As a matter of fact we know this treatment does succeed in many cases, but the *rationale* of the process is by no means satisfactory to us. And this is not a solitary instance of a successful practice following, through good luck, from a very faulty theory. If we analyze the case described we

shall find, that when the patient ceases to improve under the Nux or the Mercurius which was first given, his symptoms changed in such a way that their totality furnished a satisfactory indication for Opium, and a capable prescriber would accordingly have selected Opium and cured his case, even though the notion of "exhausted susceptibility" had never entered his head. Simple adherence to the great principle of homœopathic prescribing, viz., that each prescription is to be founded on a new collection of the symptoms and a new comparison of them with the Materia Medica, leads to success in all such cases, without the intervention of any pathological hypothesis whatever; at the same time it excludes all danger of appearing to sanction the bad habit of alternation.

As regards the use of Opium for constipation in chronic diseases, and Coffea in neuralgia, these being used as intermediate or alternated remedies, they, like topical applications in traumatic complications, are much abused, and often altogether needless if not hurtful. A single example will illustrate this point: A patient, not long ago, while under a friend's treatment came under my observation. Her symptoms corresponded exceedingly well with those of Conium maculatum. It was a chronic disease of long standing. She had troublesome constipation, which was sometimes so bad that it seemed to completely neutralize the good effect which Conium was evidently producing. A dose or two of Opium [0] would relieve the constipation, and the patient would seem for a while to improve again under the Conium. This might be called an illustration of what Dr. Drysdale refers to as the necessity for alternated or intermediate remedies in either "complications of chronic diseases," or "exhausted susceptibility." It was not, however, satisfactory to my friend, nor to myself. He could not regard the regularly recurring constipation as a foreign complication. Believing in the unity of disease, he looked upon it as an integral portion of that patient's sickness and did not rest contented until he had found a single remedy which covered

both the symptoms to which Conium corresponded and the constipation besides. This remedy was Alumina, under which the bowels became, and they have remained, perfectly regular. The patient's improvement, in other respects, was all that could be desired. In this case, as in most cases narrated of cures by alternation, the Opium and Conium, in alternation, seemed to work very well, and promised to effect a cure in the fullness of time. I doubt not that if, to all the other histories of cures by alternation, a *sequel* could be written, it would be found that each of these cases has, in the Materia Medica (actual or future), its own particular Alumina, which would effect a cure in as few *days*, as the most sanguine alternate would expect to accomplish it in *months*.

Having thus shown our belief that the alternation which Dr. Drysdale defends, for the purposes of "meeting complications," and of "awakening susceptibility," is *not* alternation *a priori*, not *alternation properly so called*, but is compatible with sound doctrine, although *explained* by the use of *unsound pathological hypotheses*, we shall pass, next, to the two general principles on which alternation, *properly* so called, is really based and defended.

Before proceeding further in the discussion of the subject of Alternation, let us briefly recapitulate the positions already taken.

1. We have assumed that the propriety of Alternation can be *argued about* only by those who regard the practice of medicine as an art based upon scientific principles, by reference to which it is to be regulated and exercised. We have not considered it possible to discuss the question with those who make their practice a series of repetitions of individual experiences, without the intervention of general principles based on and inferred from a large number of individual instances.

No discussion can be sustained with this class of practitioners, for the reason that argument involves, in its very

nature, an appeal to principle and the exercise of reason. When, consequently, an advocate of Alternation objects to our condemnation of his method, that *he* regards the subject from a *practical* point of view, while *we* persist in ignoring the *practical*, and in looking only at the question as one of *science*, we feel driven to the conclusion that, if excluded from the ground of *scientific principle*, we have no ground left on which to stand for the discussion of this or of *any* question of medical practice; and further than this we have nothing to say.

2. We have stated our belief that many writers have defended, under the name of ALTERNATION, modes of practice which are, in our opinion, not properly called Alternation, and which certainly are not open to the objections that we entertain to what *we* have defined as ALTERNATION, properly so called.

3. Defining Alternation, as we understand it, we have taken occasion to state the requirements of a sound homœopathic prescription, requirements which cannot be met by the process of Alternation.

We come now, in conclusion, to consider certain statements of principles by which Alternation, as we define it has been sought to be justified and defended.

Dr. Coe[1] states: "It is an established principle in Homœopathy, demonstrated by drug-proving and clinical experience, that each drug has its own specific sphere and manner of action, hence that each remedy acts in a particular manner upon a particular organ or tissue, or upon a particular set of organs or tissues. Another recognized principle in Homœopathy is, that attenuated remedies act on the system only by virtue of their homœopathicity to the disease by which the system is at the time affected; hence they are inert when taken by persons in health." [One of our best provings of Natrum muriaticum on the healthy was made by Dr. Wurmb with the *30th decimal dilution!*] * *

[1] *American Homœopathic Review*, vol. v., p. 447.

"But, supposing I find no single remedy that will complete the picture [corresponding to the picture of the disease]; some part is still defective—it either wants a head, a body, or a limb, what am I to do? What I do is this: I finish out the picture with something that will complete it. If the head symptoms are unmistakably Aconite symptoms, and nothing else, the chest symptoms unmistakably Bryonia symptoms, and nothing else, and the symptoms of the lower limbs unmistakably Rhus symptoms, and nothing else,[1] I can finish my picture in no other way, nor can any one else. * * But the single remedy objector says, one of my medicines will modify the action of the other in such a manner that I cannot rely upon their doing what their pathogenesis would indicate. I think he is mistaken If attenuated medicines only act homœopathically, as we all hold, only *act upon those organs and tissues* which are affected by disease in a manner similar to the drug affection, then my Aconite, Bryonia and Rhus each goes to its own place, and performs its own office, without interfering or being interfered with by the others."

Dr. Drysdale appears to agree with Dr. Coe, for he says:[2] "There are, therefore, no *a priori* physiological grounds for doubting that two medicines, whose physiological spheres are sufficiently dissimilar, can display their effects without interference, when given at intervals. Let us apply this to the treatment of complications in disease, and by this we may chiefly understand those symptoms or morbid states that are not necessarily dependent on one common proximate cause, but are connected merely by their happening to co-exist in the same individual." [We protest against this cool

[1] Humano capiti cervicem pictor equinam,
Jungere si velit, et varias inducere plumas
Undique collatis membris, ut turpiter atrum
Desinat in piscem mulier formosa superne,
Spectatum admissi risum teneatis, amici?

Horat. Epist. ad Pisones.

[2] *Annals of British Hom. Society*, xvii., 375.

assumption that two or more "morbid states may co-exist in the same individual," connected merely by their accidental co-existence!] "After exposure to cold we may be attacked with inflammation of the nasal, tracheal or bronchial mucous membrane, or parenchyma of the lungs or the pleura or the parietes of the chest or the liver or peritoneum, etc., according to the specific susceptibility of the tissues attacked; and each of these diseases may exist separately, and be attended with the appropriate essential and sympathetic symptoms of the case. In such cases we ought properly to rely on one medicine, which may be reasonably expected to meet the specific susceptibility of the part primarily affected. But, on the other hand, from exposure to the same cause, inflammation may be set up in several of those tissues at the same time, and thus several trains of morbid symptoms set up that have no necessary [would the author have better expressed his real meaning if he had said, '*pathologico-anatomical?*'] connection, except that of occurring in the same individual. In that case, how can any one medicine be homœopathic to the case which does not show its specific relation to all those *different tissues* in health? * *

"Dr. Cate also brings forward inflammation of the mucous coat of the colon. When it extends to the peritoneal coat of the gut, he gives Mercurius corrosivus in alternation with Sulphur, corresponding to their specific action on their (these) different tissues. Also in inflammation of the membranes of the brain, threatening effusion, he finds Bryonia, alternated with Hellebore, more efficacious than either singly. But this is already recognized in Homœopathy, without stepping into the doubtful regions of pathology."

In discussing these statements we desire to use great moderation of expression, conceding freely that they involve questions of pathology and pathogenesy, of which our knowledge is only, and perhaps can be only, proximative, and concerning which, instead of *absolute facts*, we have only *probabilities*.

Among those who discuss questions of medical philosophy may be distinguished two characters of mind, corresponding to similar classes among intellectual philosophers—the analytic and the synthetic.

The tendency of the one class is to divide an independent group of phenomena, such as a sick man or a drug-proving presents, into elements each one of which it inclines to regard as independent of the others. To the researches of this character of mind we owe our knowledge of Histology, both physiological and pathological, and much else that is of inestimable value in medical science. The same disposition being carried into clinical investigation, the symptoms of the sick man have been analyzed into the perversions of function of the various organs, and the alterations of the different tissues of the body, as well as the formation of tissues not found in the healthy body. As a result, we have the precious sciences of Pathology and Pathological Anatomy.

Based, as these analyses are, upon material changes of structure and of product, the danger to which those who engage in them are liable is this: that having their attention exclusively directed to material changes, or to visible and tangible results of material changes, they overlook two points to which it is equally important that observations should be directed.

1st. That inasmuch as healthy tissues exist, and healthy functions are performed, by virtue of an imponderable force called for lack of a better name, "vital force," and which is inherent in each tissue, and gives to it its specific properties, there must have been a *change in this force preceding every material change.*

2d. That this force, though it seem to impart to each tissue and organ a susceptibility to stimulus peculiar to that tissue, yet so pervades and vivifies the *entire* organism, as to give rise to what is known as *sympathy*—a property of living organisms, by virtue of which there results, from the serious modification of any function or alteration of any tissue, a

corresponding and definite modification of most if not all of the functions of the body, and an alteration of at least many of the tissues. It is by virtue of this property that each individual man is a *living unit*, and not a collection of independent monads, which merely chance to co-exist in one human form.

No physical research can detect this vital force, nor reveal the nature or *modus operandi* of this function of *sympathy*, and hence it happens that those whose minds incline to rest upon the data of physical analysis are prone to overlook them, or, even though they admit them in their philosophy, yet to practically ignore or underestimate them. From this tendency results a disposition to regard the proximate cause of disease—that is to say, the material change of tissue and product—as the *essence* of the disease, forgetting the modification of vital force, which must have preceded and been the occasion of this proximate cause,—and a further disposition to look upon the alteration of *function* and *tissue* of *each organ* or *system* of the body as a *separate* disease, thus easily admitting the idea of the co-existence of several independent diseases and of complicating diseases without number.

From the stand-point of this class of mind it is not easy to perceive the mutual relations and interdependencies of apparently remote groups of symptoms—such, for example, as *ulceration* of the *cervix uteri* and *chronic conjunctivitis*, which, if they co-exist in any patient, would be likely to be regarded as independent diseases, and to be treated by independent courses of medication; whereas, in fact, they are so intimately connected that they are best treated by a single remedy.

Nothing is farther from our purpose than to make light of Pathology, which, as *an aid in the investigation of disease*, is of inestimable value. It is evident, however, that Hahnemann was right in warning his disciples against making it the *basis of medical practice*. Its investigations cannot, in

the nature of things, go beyond *material* changes. Yet disease begins in *dynamic* changes; and the connections of different groups of symptoms are *dynamic*, and beyond the reach of physical research.

And then, even where the subject is material and amenable to physical research, to base the prescription upon a determination of the *organ* or *tissue* affected by the drug and by the disease, is to go backward from the certainty afforded by a *comparison* of the *symptoms* to the *uncertainty* of a *double hypothesis.* For, as in the cases supposed by Dr. Drysdale, we have first to assume that the symptoms produced on the healthy subject by Mercurius corrosivus and by Sulphur respectively, show that the one of these drugs acts especially on the mucous membrane of the colon, and the other on the serous covering of it; and, secondly, we must assume that, in the patient before us, the inflammation which began in the mucous membrane has extended to the serous membrane; and on this double assumption our alternation is to be based and defended. Suppose, that in its progress from within outward, the disease also affected the muscular coat—should we give Nux vomica also? Or the glands likewise—shall we give a fourth remedy, say, Rhus? Then the *vascular* system of the gut is probably simultaneously affected—would this call for Hamamelis, or for Aconite for the *arteries* and Hamamelis for the *veins?* Here we should have six remedies to be alternated, the selection of each being based upon its "*specific relation* to the *tissue*" affected, etc., etc. Now, what is all this but a revival of the old method, against which Hahnemann so earnestly protested, of prescribing upon the basis of an hypothesis of the nature and seat of the disease—an utter throwing overboard of the whole system of Inductive Philosophy, as so logically and so successfully applied to medical practice by Hahnemann?

It may be worth our while to dwell for a few moments longer on this very important branch of the subject, for it is

a point on which very many practitioners have gone astray. Indeed, we have seen an attempt to reconstruct our entire Materia Medica Pura, on the basis of the specific action of the drugs respectively upon certain organs and tissues of the body—almost all symptoms not anatomically explicable being excluded (the so-called "American Materia Medica").

The statement of a "general principle," which we quoted, viz., "It is an established principle in Homœopathy, demonstrated by drug-proving and clinical experience, that each drug has its own specific sphere and manner of action, *hence*(!) that each remedy acts in a particular manner upon a particular organ or tissue, or upon a particular set of organs or tissues," contains, it seems to us, a fallacy. While we admit, of course, that each drug has its own specific sphere and manner of action, it does not seem to us that this is equivalent to saying that each drug acts in its peculiar way on "a particular organ or tissue, or a particular set of organs and tissues," leaving—(for *this* is *implied* in the statement, as the rest of the article from which we quote shows)—leaving the other organs and tissues of the body *wholly unaffected* by the action of the drug or disease, as the case may be. Yet it is necessary to assume this fallacy, in order to justify the practice of alternation.

On the contrary, we have never met with, and we do not believe in the possible existence of, a case of sickness in which, as adduced by Dr. Coe, the *head* could be affected in one way, so as to unmistakably call for Aconite, and yet no organ of the body show Aconite symptoms; while at the same time the chest presented exclusively Bryonia symptoms, and the extremities Rhus symptoms. The uniform tendency of our own practical experience goes to satisfy us that, if any concrete case should present clear characteristic Aconite symptoms in *any portion of the body*, then not only would symptoms of disease be found in most of the other important organs or systems, but that these systems would

present more or less well defined characteristics of Aconite? or even should they not present characteristics which we recognize as those of Aconite, the symptoms would nevertheless speedily disappear under the use of Aconite, if that drug were clearly indicated by characteristic symptoms in the other organs. So abundantly has our experience confirmed this view, that if we find clear characteristics of any drug in the symptoms of any organ of the body, no matter what symptoms may be presented by other parts of the body, and no matter how little these latter symptoms may seem to indicate this same drug, we never dream of alternating remedies. We are confident that a remedy which is clearly indicated by characteristic symptoms, though they be but few in number, will cover the whole case, and will remove the entire disease. Nor do we, as is assumed by the alternators, expect to accomplish by "*succession*" what they aim to effect by Alternation. We so thoroughly believe in the unity of disease as to be confident, that, in however many organs and tissues morbid symptoms may present themselves, they still spring from and depend upon one and the same unknown and inscrutable cause, just as the multifarious symptoms of a drug-proving depend upon the one cause, viz., the drug; and that though the characteristic symptoms which furnish the indication for the remedy may be observed only in the symptoms of *one* organ or system, yet the symptoms of all the rest of the body will be equally controlled by the action of this remedy. It is remarkable, however, to how great an extent, if we observe carefully and intelligently, we may recognize, in the various groups of symptoms affecting the various organs of the patient, the characteristic mode of action, and the *conditions* of that remedy, the characteristic indications for which we find in some one organ alone of the patient!

The tendency of the second of the classes of minds into which we divided medical philosophy is to synthesis. They are, perhaps, in danger of underestimating those material

changes of tissue which are the proximate cause of disease, because they are intent upon observing and tracing out that perversion of the vital force which must have preceded and induced all the organic and material changes which the case presents, and which perversion they regard as *essentially* the disease itself. They study this perversion in its various manifestations, viz., the symptoms. They do not seek to analyze these groups of symptoms, for the purpose of forming a theory respecting the tissues affected, so as to select a remedy which affects, as they suppose, identical tissues, and in the same way. They do not thus admit *hypothesis* into their method. They study the groups of symptoms to get at their peculiarities and conditions. Profoundly impressed with the intimate connection of all parts of the body, through the all-pervading Vital Force (whatever it may be), and with the fact, observed every day, that change of function in one part of the body speedily brings about corresponding changes in almost every other part, they seek, by the light of one group of symptoms, to find in the patient other and corresponding groups. In these investigations Physiology and Pathology, which teach the relations and mutual dependencies of different organs, are of inestimable value, enabling the student to find in remote organs parallel groups of symptoms, the characteristic which determines his choice of a remedy being often in an organ very remote from that to which his attention was first called as being the seat of disease. Having thus been led, by his philosophy, to collect the various groups of symptoms presented by the entire body, as constituting one single disease, the practitioner surveys this collection in search of the characteristic symptom, or group of symptoms which shall point to his remedy. Ninety of the symptoms might be found among many remedies, ten perhaps may be peculiar to, and characteristic of, a single drug. *This* he selects, without hesitation, as his remedy for the entire malady of that patient.

Suppose the patient to have taken cold, and to present,

in consequence, a malady which, anatomically, is made up of a pneumonia and hepatitis. It is all well enough, and doubtless important for the purposes of diagnosis and prognosis, to make this pathologico-anatomical analysis of the case. But when we come to regard the case from the stand-point of Therapeutics, we are not surely to follow the plan which Dr. Drysdale seems to sanction, viz, to look among the drugs which are shown by provings to act on the tissues of the lung, and select the best from among them, and then to look among remedies which act on the tissues of the liver, and select the best from among these remedies, and to alternate the two thus selected. What if, as would be likely in such a case, the pleura, likewise, were inflamed? Should we have a third remedy? Or the kidneys also? Should we have a fourth? "*Quousque tandem* ——"

On the contrary, we should collect the various groups of symptoms, as well those from which the Pathologist infers that the tissue of the lung is affected, as those from which he infers that the tissue of the liver is affected. All other symptoms likewise would be gathered We should examine these symptoms, in the manner so often described, for the purpose of finding in them the peculiar characteristics of some particular drug. In thus dealing with the case, we should have this advantage over the Pathologist, that whereas *his* inferences may lead him astray, since the lung tissue *may* not be affected as he thinks it is—and likewise the tissue of the *liver*—we, on the other hand, taking into account only the obvious symptoms, avoid at least one very patent source of fallacy Our own experience has altogether misled us if we do not find, in the case supposed, that if the lung symptoms give us characteristic indications for a remedy, the liver symptoms not only will not *contradict this* indication by affording one for some other remedy, but they will *corroborate* the indication, so as to give us no pretext for alternating.

But another case is supposed, viz., that a pneumonia is

present, and a remedy has been well selected for it, and now a hepatitis supervenes to complicate the case. Here, we are told, is a new disease, which can have nothing to do with the previously existing pneumonia, and which *must* require a distinct treatment, in the way of alternation. This is in no way different from the complication of a burn, of which we have already spoken. If the complication is serious enough to produce constitutional symptoms (as a hepatitis would surely be), it would always (or our observation has uniformly deceived us) modify all existing symptoms. For example, a severe burn would give a typhoid character to an existing pneumonia. We must then make a new collection of the symptoms, and proceed as before to select a remedy.

Under any other plan — if we are to select our remedy according to correspondence of known drug-action and disease-action upon the *tissues* of the body — not only are we liable to errors already pointed out, but our scope is wonderfully restricted. How could we find remedies for changes of structure, such as we can never expect to see in drug-provings, such as cancer and heterologous growths of all kinds? How, for affections which do not depend on or involve any definite known change of tissue, as intermittent fever, epilepsy, hysteria, and the host of chronic ailments?

The two principles on which alternation has been sought to be defended have been stated as follows:

1st. "Each drug has its own specific sphere and manner of action; hence that remedy acts in a particular manner upon a particular organ or tissue, or upon a particular set of organs or tissues"

2d. "Attenuated remedies act upon the system only by virtue of their homœopathicity to the disease by which the system is at the time affected."

The artificial disease intentionally produced by a drug, and the natural disease which results from the usual morbific predisposing and exciting causes may, for all purposes of

argument, be regarded as identical. Principle No. 1 might, therefore, with equal propriety, be expressed thus: "A disease has its own specific sphere and manner of action; hence that disease acts in a particular manner upon a particular organ or tissue, or upon a particular set of organs or tissues."

A corollary of this proposition is, that, inasmuch as the drug and the disease respectively act in a particular manner and upon particular organs or tissues, they leave other organs and tissues of the body altogether unaffected, in their normal state and performing their healthy functions, and liable to be themselves attacked by some other disease, which may affect *them* in its own particular manner, and may run a *simultaneous* and *independent* course.

This is the argument for alternation which is constructed upon these principles: a certain disease affects in a peculiar way certain organs or tissues of the body, leaving the other organs and tissues in the fulfillment of their normal functions. Attenuated medicines may be administered to cure this disease. Attenuated medicines "act only by their homœopathicity to the disease." "They are inert" so far as action on the healthy organs or tissues is concerned. Homœopathic medicines act only where, says Dr. Drysdale, they find that preternatural susceptibility to their action, which inheres in organs or tissues diseased in a manner similar to the morbid state which those drugs can produce on the healthy.

These attenuated medicines then may be given for the case we have supposed. They will act upon the diseased organs and tissues, but will not affect the *healthy* ones at all. Now, then, during the existence of this disease, which, as stated, affects in a particular manner certain organs and tissues, and leaves all the rest undisturbed, a second disease may attack the individual (may we call one who may be thus divided up an *individual?*) acting in some other particular manner, upon some other particular organ or tissue.

To meet this new disease, co-existent with the original, it is affirmed that a second remedy homœopathic to it may be administered simultaneously with the former remedy. Or rather, since to administer it simultaneously might involve the risk of the chemical reaction of the drugs, the remedies may be alternated. It will not interfere with the former remedy, because attenuated medicines act only by virtue of their homœopathicity to the disease (to which they are respectively homœopathic). Thus each drug "will go to its own place," like the respective members of a well-trained coach team when the winding horn announces that the coach is ready for the new relay!

Such is the argument for alternation succinctly, and, we think it will be conceded, fairly, stated. In a former discussion of this subject we showed that, assuming natural disease and drug disease to be, for the purposes of this argument, substantially the same thing, it was *proving too much* to demonstrate that two or more diseases co-existing in the body could not and do not affect each other. This would render a cure of a disease impossible; for in the act of curing we propose to cure a *natural disease* by creating in the body a *drug disease* which shall annihilate the natural disease, and shall, in that very act, be itself annihilated. When a certain form of rheumatism exists in the body, we give Bryonia, and our intention is that Bryonia shall so act upon the body (producing, therefore, virtually, a *Bryonia disease)* as to cause the disappearance of the rheumatism and at the same time not to leave in its stead a Bryonia disease nor any other disease. But if two diseases co-existing cannot react upon and modify each other, how could a cure take place? The proposition is evidently too broad; the argument proves too much.

Dr. Drysdale provides against this objection by limiting the alleged possible independent co-existence of diseases, to diseases which act in a dissimilar manner upon remote or unrelated organs and tissues.

According to this view, two diseases might co-exist and not modify each other, if seated in organs distant from each other or, which is equivalent, a natural disease may exist, and a drug may be given which acts only upon organs remote from those which are the seat of the disease, and (if the doses be strong enough) may produce its peculiar drug disease without at all affecting the natural disease which is already present.

In other words, it is only diseases (whether natural or drug diseases) which are homœopathically related to each other, that are incompatible and may not co-exist without modifying each other.

Dr. Drysdale and several other physicians hold these views, and appeal to their own observation and experience. If we may be allowed to draw an inference from Dr. Drysdale's remarks,[1] he believes that a hepatitis may supervene upon an already active pneumonia, and that each may run an independent course, and be treated independently by alternated remedies, without modifying each other or blending into one morbid state.

Our own experience has led us to very different conclusions, and we have on our side the names of colleagues who, not having deviated from *the modes of practice* observed by Hahnemann's *early disciples*, do not feel compelled to acknowledge and lament, as Dr. Drysdale does, that their practice is "not proportionally so successful as was that of Hahnemann's early followers.[2]

How can this direct contrariety of inferences from observation be explained? We have altogether too profound a respect for the observing and reasoning powers of our dissentient friends, to set them a particle below our own! There must be an opposition in some of our methods of observation, or principles of inference. It will be found, we think, in the fact that to the word "*disease*" we respectively attach

[1] *Annals of British Hom. Soc.*, No. 17, p. 375.

[2] *New Repertory*, Introduction.

very different ideas. *We* mean by disease the aggregate of those symptoms presented by the sick man, which are characteristic of his particular deviation from a healthy state. The colleagues from whom we differ speak of disease as denoting a definite pathological and pathologico-anatomical change of the functions and tissues of some definite organ or system of organs.

In this latter view the patient might present evidence of those pathological changes in the organs and functions of respiration, to which the name pneumonia has been attached. Here then is *one* disease, pneumonia.

He may also present evidences of those pathological changes of function and tissue in the liver which give rise to the name hepatitis. And here would be another, co-existent disease, hepatitis; and these two pathological and pathologico-anatomical conditions might, we freely admit, from the pathological and nosological stand-point, run their course to resolution or to destruction without so modifying each other as to coalesce or in any way blend or be confounded; for the lungs and the liver can never collide.

But, let it be borne in mind, we are discussing a question of practical medicine and not one of pathology or of nosology.

How would this case look from *our* stand-point? The patient has, we suppose, dyspnœa, stitching pain in the thorax, hard, dry cough from tickling behind the sternum, scanty and occasionally bloody sputa—headache on the vertex, and sharp fever. He is worse at night; his pains are much aggravated by motion, and are relieved by repose and by warmth.

We have selected Bryonia for him—had previously diagnosticated pneumonia (physical signs aiding or confirming our diagnosis). Now he gets, in the hepatic region, sticking pains, fullness and tenderness; he has bilious vomiting and bitter taste. Aided again by physical signs, we diagnosticate hepatitis, complicating the pneumonia; but the

symptoms still indicate Bryonia, and so we continue that remedy.

But, we shall be asked, suppose the new symptoms do *not* indicate *Bryonia*, but, on the contrary, some other drug, will you not give that other drug for the liver and continue the Bryonia for the pneumonia?

To this we reply that in so far as our observation has taught us, in such a case the supervening of these new symptoms (of the liver, namely) will have so modified the whole organism, including the diseased respiratory organs, that *Bryonia will no longer be indicated* by any symptoms; but the aggregate of characteristic symptoms of the entire sickness will now indicate some other remedy which will apply to and will cover the whole morbid state of that individual.

Here we are again at issue. Dr. Drysdale and his friends appeal to certain observations which we will now briefly discuss.

In the discussion which followed the reading of Dr. Drysdale's paper in the British Homœopathic Society, Dr. Russell took the same ground as Dr. Drysdale. He said: "That two morbid specific actions could occur simultaneously in the human body, and each pursue its course without arresting or modifying the other, is a proposition entirely at variance with the opinions of the old pathologists who flourished before the time of Hahnemann. * * It is of great importance to us to know whether this pathological doctrine be in accordance with the larger experience and observation to which we now have access."

We could hardly have two diseases more specifically distinct than typhus and small-pox, yet, in the following narrative, we have a description of the two running their course, side by side, without either interfering with the other." The case occurred in the London Fever Hospital, and is quoted by Dr. Murchison.

Dr. Russell proceeded to say: "There are many instances

of a similar kind on record, and we must frame our theories so that they shall embrace this new category of cases. Suppose we encounter a case, and there are such on record, of a combination of scarlet fever and of typhus, how are we to deal with it? Are we to engage only one of the two destroying agents, and let the other alone until the first be entirely subdued? If we believe it is impossible for two medicinal actions to proceed *pari passu* in the animal economy, this is the rational course to pursue, and the one recommended by Hahnemann!" (This is an inexcusably lax statement. Hahnemann nowhere ever recommended that we should make only a part of the patient's ailment the basis of our choice of a remedy; he always strongly insisted on our considering the "*totality of the symptoms*," and this would include both scarlet fever and typhus.)

But, if two entirely different *natural* morbid processes can co-exist in the human body without the one affecting the development of the other, what reason is there why there may not be two artificial simultaneous series of morbid phenomena, each equally independent of the other? If scarlet fever, or if small pox, can each run its course, while at the same time typhus is doing so likewise, why may not Belladonna and Arsenicum each run their course when given in alternation."[1]

In reference to this last question, we may ask, would Dr. Russell expect to get pure symptoms of any drug if it were proved simultaneously with another drug; would he believe that the two drug-diseases would run an independent, simultaneous course in a prover? Would he practically believe it by consenting to rely upon a materia medica pura made up in this way? Or, which is the same thing, would he trust to a materia medica composed of the results of an artificial (drug) disease running "an independent course simultaneously" with "a natural disease?" In other words, would he trust provings made on sick persons?

[1] *Annals British Homœopathic Society*, xvii., p. 399 *et seq.*

We know he would not; for he is one of those who reject or suspect the provings of many of our most valuable drugs, because symptoms observed on the sick are included in the list, or because the provers were not careful enough to exclude other morbific or toxic agents while proving.

But this inconsistency aside, let us take notice that in these remarks, and in this citation of a case of the co-existence of two diseases, Dr. Russell speaks altogether from the stand-point of the pathologist and nosologist, and not at all from that of the practical physician—the stand-point of therapeutics. He thinks he has proved his case if he has established, what nobody is disposed to deny, viz., that two nosological abstractions, called by distinct names (to wit, scarlet fever and typhus), may co-exist.

Does he forget that nothing is more firmly established in homœopathic practice than that the "*name*" which we may feel authorized to give to a sickness in no wise determines our *treatment* of the sickness? Because we call the sickness typhus, we do not *therefore*, of necessity, give Arsenicum, or scarlet fever Belladonna! We may as often have to give Arsenic in a case that we should call scarlet fever, and Belladonna in a typhus.

What, then, guides us in selecting a remedy? Nothing save the aggregate of the characteristic symptoms. We might divide and subdivide the symptoms and signs that the sick man presents (including under the designation "symptoms and signs" everything that distinguishes that sick man from himself when not sick) into a dozen nosological groups, each having a distinct name, and yet the aggregate of these symptoms might (and we think it always would) point to one single remedy as corresponding to, and indicated by, the entire morbid state of that sick man.

To prove his case by the argument and instances quoted, Dr. Russell must show that the symptoms of the scarlet fever and of the typhus, respectively, were incompatible in

the case cited by him, and that they did not combine to present the characteristics of one common remedy.

For this is the point of the whole discussion. No matter how many of the pathological groups which are dignified by the names of distinct diseases may co-exist in a patient, if they be so blended and mutually modified as to indicate, in the aggregate, one single remedy, there can be no call for *alternation.*

Inasmuch as Dr. Russell rests his argument for alternation on the possible and frequent co-existence of distinct, specific forms of disease, it is incumbent on him to show that these co-existent, distinct forms of disease do not, by their co-existence, modify each other and blend into one harmonious aggregate of symptoms, the characteristic ones of which may be covered by a single remedy. In our experience this has always seemed to be the case, and we shall adduce other evidence to the same effect.

But before citing authorities, let us point out how far Dr. Russell's argument, even allowing it full force, comes short of being a defense of alternation, as it is actually practiced here and in England! Dr. Russell allows it where "two distinct, specific forms of disease" co-exist, such as scarlet fever or small-pox and typhus—a phenomenon that no physician in ordinary family practice is likely to observe more than once a week! Yet how *many times a day* do a majority of our colleagues alternate? It is safe to say that, with very many, the *giving a single remedy* is the *infrequent exception!* Their case is exactly met by the following remarks of Dr. Russell, one of the *champions* of alternation:

"The objection usually urged against alternation is that it leads to laxity of practice. True, if we give two medicines instead of one and let the system take its choice, as it were, to which it shall submit—if, in a given case, for example, we find two medicines pretty nearly indicated, and, instead of ourselves selecting the one and rejecting the other, we toss them both in, trusting that the right one will act,

and the other be a nonentity or negative quantity. NO ONE WHO DEALS CONSCIENTIOUSLY WITH HIMSELF WILL DELIBERATELY APPROVE OF SO SIMPLE A METHOD OF EVADING THE DIFFICULTY OF CHOICE."

A few instances and citations may serve to recall to the minds of practitioners of medicine, the frequent instances in which two varieties of disease, co-existing, modify each other. A young person whose parents died of consumption, and who has himself had abscesses of the cervical glands and eruptions of the face and who takes cold readily—is feeble and pale—who, in short, has scrofulosis and is a predestined victim of consumption, gets an attack of enteritis or of pneumonia. Is it not notorious that the joint action and reaction upon each other, of this chronic scrofulosis and the acute enteritis or pneumonia, will be such that instead of the acute affections running the short and sharp course which they would follow in a robust subject, they will almost surely degenerate into tuberculosis of the mesenteric glands with colliquative diarrhœa, and of the lungs, respectively? And would any one dream of treating these acute affections in such a subject just as they would treat them in a subject otherwise and previously healthy?

But why not? Because the previously existing disease modified and (so far as symptoms are concerned) blended with the acute attacks. No doubt *one single remedy* will cover this blended case.

When a patient has suffered from paroxysms of intermittent fever, and on the cessation of their regular recurrence he is still sallow, feeble, dyspeptic and full of malaise, there can be no question that he still suffers from the disease—the result of marsh miasm. If now this patient get a pneumonia or a neuralgia, here will be a second and a distinct and dissimilar disease co-existing with the former. So different, however, will be its course from what it would have been in a patient not laboring under miasmatic disease,—so blended will it be, from the beginning, with the latter,—that, though

a wholly distinct "pathological form" of disease, it will require the identical treatment of miasmatic disease and will yield to nothing else. The same thing is eminently true of *syphilis* in the system.

Bonchut says:[1] "Syphilis, herpetism, scrofula, the marsh-miasmatic disease, etc., often appear under forms not habitual. * * * We imagine that we are dealing with acute or chronic inflammations, and we treat them as such until, some day, changing our method, we see them recover, one under Mercury, another under Sulphur, another under Sulphate of quinine, another under Iodine, etc. Is there anything stranger than those observations in marshy countries, where pneumonia, fever, encephalo-meningitis, are cured by Quinine rather than by blood-lettings and by antiphlogistics?"

Another instance of the modifying power of two morbid influences co-operating on the same individual is furnished by the fact that when an epidemic prevails not only are almost all healthy persons, to some extent, affected by the epidemic influence, but the maladies of all sick persons, whatever their nature may be, are modified and changed into a greater or less conformity with the epidemic, whatever it may be.

Dr. David Hosack uses these words:[2] "The fact stated by Sydenham and other writers on epidemics, that the prevailing disease swallows up all other disorders; that is, that during the prevalence of an epidemic plague, typhus, dysentery or other diseases of this class, every indisposition of a febrile sort readily assumes the character of the prevailing disorder. We know this to be experienced in the diseases of other countries, and we see it daily exemplified in our own."

Homœopathicians especially, since their observations are finer and are less exposed to fallacy from drug-poisonings,

[1] *Pathologie Générale*, p. 269.

[2] *Copland's Dict. Pract. Med.*, 2, 404, note.

have opportunity to make observations of this kind, and they have often noticed how, at one season and under one epidemic, a single drug or group of drugs corresponds to the most diverse pathological forms of disease, while at other seasons and under other epidemic influences, very different groups of remedies are indicated by the same pathological forms of disease.

And we believe that most Homœopathicians who have dealt much with chronic cases, must have fallen in with patients who labor under such a constitutional affection as herpes or cancer, and which, under favorable circumstances, is kept in a latent condition by the occasional use of some clearly indicated remedy. If these patients get an acute disease of whatever kind, this disease is almost sure to present, at an early period, symptoms which indicate this same remedy that suits their *chronic* malady. Does not this argument *a posteriori* show that these two independent, co-existent maladies blend and modify each other, at least in the view of the *therapeutist?*

The entire homœopathic doctrine of chronic diseases, and the use and necessity of so-called antipsoric remedies, are based upon this same idea of the mutually modifying influence of two co-existent maladies.

Hahnemann says he found that in some persons acute disorders did not yield promptly to remedies which seemed indicated and which quickly cured them in others. On examining closely he found these patients presented, in the present or in the past, symptoms indicating their infection by certain miasms which he reduced (whether rightly or not is immaterial) to three miasms—psora, syphilis and sycosis. He sought for remedies which would meet the indications of these chronic miasms and the acute affections combined, and this led him to prove and use the remedies known as the antipsorics. Our success with those remedies might be taken as another *a posteriori* proof of the blending of two co-existing diseases.

Dr. Drysdale, apologizing for the practice of alternation, says:

"We have not a few examples where the patient has, through ignorance or design, unknown to us, taken large doses of heterogeneous drugs, such as Quinine or Opium or purgatives, and yet our higher dilutions have taken effect notwithstanding. On this subject Trinks confirms Kampfer's remark, that, in chronic diseases which have been long treated with excessive doses of allopathic medicines, we often find a great susceptibility to the action of homœopathic medicine."

Are these writers to be understood as maintaining that chronic drug-poisonings, such as the mercurial, the sulphur, the iodine, the quinine, the iron or the opium cachexy, do not cause acute disease to be most difficult of cure? The contrary is notoriously the case, as the literature of physicians of every school abundantly shows. How can this be, unless the *artificial* chronic disease, viz., the cachexy, blend with, and modify, the supervening acute, *natural* disease!

These remarks might be extended to great length by multiplied instances. They suffice, however, to show distinctly the practical conclusions to which, what we conceive to be sound principles, as well as accurate observation, have led us. With them we conclude what we have to say concerning the alternation of remedies, taking our leave of the subject in the spirit of Bishop Chillingworth's declaration:

"I will take no man's liberty of judgment from him; neither shall any man take mine from me. I will think no man the worse man, * * I will love no man the less for differing in opinion from me. And what measure I mete to others I expect from them again."

EDITORIAL REMARKS ON ALTERNATION.[1]

I.

We publish with great pleasure articles which, like the above, though they seem to favor a practice which we deprecate, are, nevertheless, honest expressions of the earnest convictions of men of close observation and faithful study.

Nothing could be farther from our intention than to undertake to dogmatize on this question, which we admit to be an open question, and on which men of eminence entertain opinions at variance with our own. It may be allowed us, nevertheless, in all courtesy to our contributor, to signify the points on which we take issue with him.

1. We assuredly do entertain the belief that "two or more medicinal forces (or, we prefer to say "morbific forces") cannot together act upon the economy without so modifying each other that neither shall produce the effect it would if only one were acting." This belief has prevailed among medical men from the earliest ages It is the foundation of the practice of polypharmacy in all its varieties, from the complex prescriptions of the seventeenth century to the alternations of our own colleagues. For we hold, with Dr. Sorge (by no means a Hahnemannian, let us add), that "the practice of alternation, as it exists among homœopathicians, is only another form of *mixing remedies* with the intention of getting an effect compounded of the action of the two or more drugs that are alternated."

We say, this belief is the foundation of polypharmacy. The *rationale* of a compound prescription we take to be the following: the drug which is regarded as the one chiefly

[1] Suggested by a contribution by Dr. H. to the *American Homœopathic Review*, vol. v., February, 1865.

indicated possesses, let us suppose, certain properties which would be hurtful to the patient. Another drug is conjoined with it for the purpose of antidoting those hurtful properties. Again, it lacks the power to produce certain effects which are deemed desirable. Another drug is added to supplement this deficiency, and so on, *ad infinitum*. This entire procedure rests on the belief that these medicinal forces will so modify each other in the economy that neither shall produce the effect it would if only one were acting.

As an example, we mention calomel and opium, a very familiar combination. Certainly the object of giving the opium is that it may modify the action of the calomel, and in a way which conforms to the known action of opium when given alone.

If there were no reason to believe that "two or more morbific forces (a *drug* is a morbific agent exciting a morbific force) could not together act upon the economy, without so modifying each other that neither shall produce the effect it would if only one were acting," how could we ever *cure disease?* The morbific force which constitutes the disease is acting upon the economy, and we bring to bear upon the economy another morbific force, viz., a drug. Now, if the latter be properly selected with reference to the former, we know that these forces will so modify each other that, on the one hand, the manifestations of disease will cease, and on the other hand, the drug will not produce pathogenetic symptoms as it would on the healthy subject. This statement rests on the basis of such "facts" as our friend invokes. On what other theory than the mutual modification of morbific forces acting together upon the economy could it be explained?

On what other theory could we explain the action of antidotes as homœopathicians understand and employ them? How else could we explain the action of Belladonna, in large doses, in antidoting poisonous doses of morphine[1] as

[1] *American Journal Medical Science*, January, 1862.

well as the mutual antidoting powers which allopaths have recently discovered, many of the cerebro stimulants and spinant alkaloids to possess? How else, finally, could we explain the efficacy claimed for the practice of alternation in general and for certain instances of it in particular, such as the cases related by Dr. H., and which we fully credit, where Belladonna and Bryonia in alternation accomplished what neither could do singly, and where Arsenic and China in alternation cured a case which neither alone had cured? How else but by conceding that two or more medicinal forces cannot act together upon the economy without so modifying each other, that neither shall produce the effect it would if only one were acting?

The fact that medicinal or morbific influences must be avoided when we are engaged in proving drugs is universally admitted; even Dr. H assented to it But on what other grounds than the belief already stated?

That persons long accustomed to use tobacco or coffee are not easily affected by drugs is accounted for, probably, by the fact that long habit has, in so far as those persons are concerned, caused tobacco or coffee to cease to be "medicinal forces" Use has become "second nature." That in persons not accustomed to the use of coffee, a dose of it will modify the action of another morbific, or medicinal, force is demonstrated by the effect of coffee antidoting opium, Nux vomica, alcohol, as well as in neutralizing the beneficial curative action of many drugs, as our clinical observations often satisfy us that it does The same statements apply to tobacco.

2. We think we may then assume, as conceded, the fact that "two or more medicinal forces cannot at the same time act upon the human system, without mutual modification." It is now asked, whether granting this modification, we may not avail ourselves of it to derive, from the alternate use of drugs which modify each other, good effects that we have attained in no other way? There can be no doubt of the

possibility of this being done, no doubt that it has been done with advantage; as, indeed, Dr. H. shows by two instances.

But, in the present state of our science, it would be impossible for any one to lay down rules for the selection of remedies to be given in alternation, with the view of gaining any desired modification of the action of each. Nor can we conceive of the possibility of arriving at any such law. The rule of the ancient polypharmacy, viz., to select remedies according to the effects which they produce singly, and to combine them with reference to the effect desired, does not always work well in practice. For example, calomel and opium produce a combined effect, which is clearly a modification of the known action of each drug. But on the other hand, the combined action of opium, ipecacuanha and the sulphate of potash in Dover's powder is quite distinct and different from that of either constituent alone, and is such as could hardly be anticipated from what we know of these constituents.

Arsenic and China produced a combined effect which cured an ague that neither singly would cure. Now, if this fact were to be taken as a guide in selecting a combination for a case of uterine disease, for example, in which we were at a loss to decide between Sepia and Pulsatilla, it might lead us to alternate those two remedies. But it is not three months since one of us took charge of a case in which this alternation, on this very ground, *had* been employed, but the patient had not recovered. We were led to the conclusion that the Sepia was really the remedy indicated—that it did good, but that its effects were constantly antidoted by the Pulsatilla. We omitted the Pulsatilla, gave Sepia judiciously, and the patient promptly recovered.

When, in addition to this difficulty arising from the fact that alternated drugs do not always produce a morbific action which can be predicated in advance from their known single action, we consider that the various degrees of suscepti-

bility of the organism to drugs indicated with different degrees of accuracy, will cause the modification by drugs of each other's action to be different in each individual case, we clearly see that we can never be in a position to predicate, in advance, the effect of the joint administration of several drugs. We can never, therefore, have a scientific indication for such a combination or alternation. If we make it, it must always be the result of "guess-work"—of "feeling our way along," just as Dr. H. did in the cases he relates.

But we claim superiority for Homœopathy in that it gives us the means of selecting our remedies with a reasonable certainty of their effects, and we rightly claim that medicine should not rest content with anything short of a method which, given the symptoms of a disease, points us to a *certain* remedy if our materia medica contain it; or, given the pathogenesis of a drug, indicates to us, *a priori*, the complex of symptoms which the drug will remove.

But the method of alternation throws us back from all such certainty—back to the field of unmethodized clinical experience—back to the trackless wilderness of polypharmacy and allopathy.

For these reasons, briefly and imperfectly stated, from the stand-point of science we hold that homœopathic medicine cannot countenance nor tolerate alternation. And we have such a faith in the unity of nature as to believe that what sound reasoning shows to be erroneous, accumulated facts will prove to be mischievous and unnecessary.

3. But there is another view of this matter from the *practical* stand-point. Dr. H. says: "Grant that in a given case of the use of two remedies, some one remedy might have been found that would as well, or better, have answered the purpose * * * the question is not, Could some other person have done better than I, but Have *I* done the best I could? Undoubtedly, yes! And, so far as the prescriber's own conscience is concerned, this answer exonerates him, even though his prescription were a faulty

one. And in so far, from the *practical* point of view, alternation may be justifiable. But will this question, 'Have I done the best I could?' if affirmatively answered, always justify the prescriber? The practitioner of one year's experience may commit a sad blunder and yet be fully justified, because being inexperienced and young, bad though his error was, he did 'the best he could.' Would this plea justify the same blunder ten years later? Obviously it would not. Yet is it not the tendency of this plea to satisfy a man with the knowledge he has and with the methods he pursues, and thus to blunt his zeal for greater knowledge and better methods? Remonstrate with the allopathist for murdering patients with lancet and purge; he replies, 'I act according to my light; I do the best I can.' And so through all degrees of error and imperfect knowledge. It is a dangerous plea, unless its complement be always added, 'I have done the best I can, but, please God, I will do better next time!'"

And here we see the advantage of a *sound* "*theory*," which our friend holds in so light esteem.

Suppose a practitioner driven, as Dr. H. was driven, to alternate. The patient recovers. He has, nevertheless, a conviction that drugs modify each other's action in a way that we cannot foretell, and that the action of the entire organism is so harmonious that "two morbid states cannot co-exist independently," and that, therefore, the morbid state being essentially *one*, there must be a possibility of one similar drug-disease to oppose it. A sound theory compels him to the belief that alternation is not justifiable nor defensible on scientific grounds, and that it can do nothing to advance our knowledge of Therapeutics, but rather confuses it. He enters in his mind a protest against the practice to which the necessities of practical duty and his limited knowledge of the materia medica have constrained him, and turns with redoubled vigor to the study of materia medica, smarting a little with shame that the exigencies of practice

have found him unprepared, and have compelled him to a resort which, though successful, his reason condemns, and determined that the next time he will, if possible, be equally if not *more* successful, and by a method which shall commend itself to his reason, and shall add to his stock of knowledge for future use.

And if, while "doing the best they can," as each case presents itself, practitioners will earnestly and systematically study the materia medica, determined to do better and better the next time, we shall have no word of reproach to utter against their temporary expedient of *alternating drugs.* With broader knowledge they will alternate less and less, and we are very sure that they will see, as we have done, how, in case after case in which they had alternated, the recovery was in truth retarded by the mutual reaction of the drugs; and how the careful individualization of the case, and the painstaking selection of the single drug in the manner that Hahnemann advised, will be in their hands the means to a success far beyond any ever reached through alternation. For, though we have admitted and do admit that success is often attained through alternation, and that "success is the object of all our labors," we are nevertheless convinced, through our own experience,—for we have alternated in our day,—that a much larger measure of success is obtained by adherence to the rule of administering single remedies, and that closer study of the materia medica and sharper investigation of cases will preserve us from those dilemmas in which we are tempted or compelled to resort to alternation.

4. And if we appeal to the experience of Hahnemann, let it be understood that we appeal to the man who both knew materia medica better than any other man ever knew it, and who had more practical experience than any of us has had. Let us not indulge in the fallacy of supposing that, because we are now practicing in the fifty-ninth year since the *Organon* was published, we have had fifty-nine

years' experience in homœopathic practice, and have been studying materia medica fifty-nine years. On the contrary, our own experience is measured only by the actual number of years we have practiced; since in this matter of assimilating to one's own mind the facts of the materia medica, and of seeing the correspondence between these and the facts of the disease, we can borrow but little from the experience of others.

If then we, from our little experience of fifteen, or ten, or five years, appeal to Hahnemann with his venerable experience of more than fifty years of active practice, with his unapproachable knowledge of the materia medica, of which he might justly say like Æneas, "*magna pars fui*," with his unrivaled powers of observation and discrimination; if we appeal to him as "authority" on this question, at once practical and scientific, can it justly be said, that we are seeking some "*authority* outside of and beyond our own reason?" Our colleague appeals to collections of "facts." Is not Hahnemann's statement of his practical conclusions a most stupendous "collection of facts?" Who ever observed so many of them? Who ever observed so well as he? Facts must be received on testimony; who ever reported more graphically and more faithfully than Hahnemann? If we doubt his ability, his capacity, his candor, what are we doing with his materia medica, on the truth of which we risk our patients' lives?

This outcry against "swearing in the words of the master" has come to have a very different meaning from that of the ancient original protest. It was never meant to intimate that the opinion and testimony of him whose abilities had crowned him "king of men" should not have a royal weight of influence.

EDITORIAL REMARKS ON ALTERNATION.[1]

II.

This article very justly affirms that the question of alternation, being a practical one, "must be decided by experience;" that no dogmatism will satisfy those differing in opinion. The writer then straightway proceeds to dogmatize on the basis of two assumptions, which he claims to be "established principles in Homœopathy," but which very few will concede in the terms in which he has expressed them.

Having referred the question to experience, he makes no appeal to experience, whether his own or his neighbor's. If he have experience which can throw light on this question, it ought to be comparative experience. He ought to be able to say: "For a certain period of times I avoided alternation, selecting my remedies as Hahnemann directs; then, for a period I alternated. My success under the latter method was the greater." Such a statement as this would carry weight. But we gather from this article that the author has always practiced alternation, deriving his ideas of its propriety and necessity from the assumed "established principles" upon which he bases his arguments, and upon the assumed imperfections of the materia medica.

If our conjecture be correct, then his experience, being confined to the practice of alternation and not bearing upon the opposite method, can have only a negative value. It merely shows that a measure of success does attend this faulty form of practice, a point which was freely conceded to Dr. H.[2] But inasmuch as no advocate of alternation claims invariable success, the question remains, "Might not the

[1] Suggested by a contribution by Dr. C. to the *American Homœopathic Review*, vol. v., April, 1865. [2] See previous paper.

success have been greater if alternation had been *avoided?*" Not having any comparative experience by the light of which to answer this question, the alternator *ab initio*, could not reply.

Our editorial note to Dr. H.'s paper expressly referred to Hahnemann's experience, and to the experience of the writer, as being of this comparative character, and as, therefore, bearing on this question, and as showing, so far as they go, that the results of adherence to the single remedy are more satisfactory than those of alternation.

Some of the points touched by Dr. C. are too important to be treated in this annotation. They will be discussed in a future number of the *Review*.[1]

Suffice it now to say that, when he states that "the only reason why he practices alternation is the want of perfection of the materia medica," he concedes (if he will allow us to add one other analogous reason, viz., "want of perfection in the knowledge possessed by practitioners of the materia medica which we already have") all that the opponents of alternation contend for. For this position admits that alternation is wrong in principle, and is only necessitated by a temporary emergency. We contend that the emergency, instead of being common, is rare, if not altogether imaginary, and we appeal to our practice.

How dangerous it is to discuss a question of science with rhetorical figures!

Dr. C. makes merry over his imagined double-headed monster, for each head of which he provides a club, and he grows happy at the thought that by his two-handed energy he will have destroyed this figurative disease much more quickly than the luckless editor could do it, whom he restricts to the use of a single club to be applied to but one head at a time.

Now, we have always supposed that the efficacy of an armament depended not so much upon the number of

[1] See "Alternation of Remedies," Nos. 1 and 2.

efficient weapons as upon the skill with which the weapons existing were used, and one club in a vigorous right hand might do more execution even on a double-headed monster than two clubs even in the hands of our ambidextrous correspondent. But why restrict the monster to the "two-headed" form? If we are to admit the dual or multiple independent co-existence of disease, why not liken it to a centipede, each foot "possessing independent vitality?" With what armament now will our friend cope with his antagonist? Shall he call Briareus in consultation, and arm him with one hundred clubs? We do not believe in the multiple co-existence of disease. Leaving out of view traumatic affections which may supervene during idiopathic sickness, as Drysdale shows, we believe in the unity of disease, and we expect to find one remedy of which the characteristic symptoms cover the whole case.

The figure of the two-headed monster is, therefore, in our view, defective, but, admitting the figure, why must we use clubs at all? In the days when brute force reigned supreme they were well enough. But surely in this day of enlightenment "clubs are not trumps."

Hercules was the embodiment of brute strength as distinguished from intellectual acuteness and skill. Surely his subjugation of the Lernean Hydra should not serve us as an example in treating disease. We cannot afford to spend so much time as the club treatment requires; for both the experience of Hercules, and that of our Irish friends at Donnybrook Fair, prove that the breaking of heads by clubs is a tedious and uncertain affair. We decline to enter into competition with our friend in the use of his favorite therapeutic agent, the club, whether he use it single, double, or a hundred fold. But, if he *insist* on a mechanical figure, we will suggest that, while *he* is counting the heads of the monster and is casting about in his club-room for a bludgeon adapted to each head, and is fitting his hand, for the arduous effort, and is so getting his balance as to make sure that the

complex wielding of his numerous weapons shall not trip him up, *we* shall humbly survey the monstrous form with the intent to study the controlling characteristics of his anatomical structure, and when we shall have learned to what type he belongs and what are his *characteristics* as an *individual*, then with one *single* weapon (a slender sword, it may be, or a bodkin, or a drop of prussic acid), we shall touch the vital part (the heart, or the medulla, or a delicate mucous membrane) on whose integrity depends the life of all the heads and all the rest—and presto—before the ponderous clubs have time to fall on those devoted heads "belaboring them simultaneously"—the work is done!

Multiplicity in armament is generally inconsistent with simplicity and efficiency, and is characteristic of a barbaric age or race.

Thus the double-club array is known as the "Indian Clubs."

The Japanese wear two swords, and yet cannot stand a moment before the single rapier of the Frenchman.

The savage of the Southern States glories in his belt full of six-shooters, and his boot-legs bristling with bowie-knives. Before the *single purpose* of the Northern Farmer he is rapidly passing into the realm of history.

THE USE OF HIGH POTENCIES IN THE TREATMENT OF THE SICK.

Having been honored by the President of the Society[1] with an appointment to report on the use of High Potencies in Chronic Diseases, I trust it may not be considered an unwarrantable presumption if, for the following reasons, I venture to modify, to a slight extent, the theme assigned to me

First. The subject contemplates a report of a practical nature. But from the stand-point of practice, I think that a clear and well-defined distinction between acute and chronic diseases is not possible. If we attempt to base such a distinction upon the element of time alone, paying regard only to the *duration* of the disease, it cannot be made at the beginning of the treatment, and cannot therefore be of any practical value in determining the potency to be used.

If we base the distinction upon a pathological conception —that is, upon the presumed existence in the patient of some dyscrasia, diathesis or miasm—it is equally impossible, at least in very many instances, to make it at the commencement of an illness.

It is a matter of common remark that cases which begin without any sign of miasmatic or dyscratic complication often develop in their course unequivocal evidence of such complication; so that a case which would, in the outset, have been regarded as, in this sense, unquestionably *acute*, proves in its course to be unmistakably *chronic*

Second Furthermore, the subject as stated, involves a kind of *petitio principii.* To require a report on the "Use

[1] The Homœopathic Medical Society of the State of New-York, Oct., 1863.

of the High Potencies in Chronic Diseases" is in some sort to imply that these potencies are especially useful and appropriate, if at all, in the treatment of *chronic* diseases as distinguished from *acute* diseases—an implication which it is true corresponds to a very widely received opinion among those physicians who have but little practical acquaintance with the action of the high potencies, but which, as we shall see, is not supported by the results of experiment.

By the kind indulgence of the Society, then, I shall beg leave to report on the "Use of High Potencies in the Treatment of the Sick."

No question connected with Homœopathy has given rise to more vigorous controversy or to more earnest partisan feeling than that concerning the infinitesimal dose. The most bitter opposition of the Old School to Hahnemann was based on this question. It has been also the chief ground of division and contention among homœopathists themselves.

After an animated disputation covering a period of more than fifty years, there seems now, however, to be a cessation of hostilities between the advocates and opponents of the higher potencies. There seems to be a mutual disposition to suspend the discussion hitherto maintained chiefly on hypothetical grounds, and to appeal in good faith to the "*ultima ratio*"—experiment. The times are favorable, therefore, for a quiet survey of the field and for a dispassionate estimate of the experimental knowledge of which the practical labors of our colleagues have put us in possession.

It is probable that Hahnemann was never what could be called an "heroic prescriber." In his work on *The Venereal Disease*, published 1789, before he had discovered the homœopathic law of cure, he portrays the evil effects of excessive doses of mercurial preparations, and speaks of radically curing some cases of syphilis by the use of a single grain of

Mercurius solubilis in divided doses; and says that eight grains thus administered will often be sufficient to cure "very severe cases of syphilis." Compared with the practice of his contemporaries, these are almost infinitesimal prescriptions.

In his first essays upon the homœopathic law, Hahnemann advises the administration of doses but little, if any, smaller than those he had previously used. We first meet with the recommendation to use infinitesimal doses in the essays on "Scarlet Fever," published in 1801, but referring to cases treated in 1799. He here advises Belladonna and Chamomilla to be given in preparations which correspond pretty nearly to the third centesimal dilution.

It pleases Dr. Dudgeon to suppose that "this sudden change," as he calls it, from material to infinitesimal doses, was a matter of expediency and policy rather than of conviction on the part of Hahnemann, inasmuch as it coincided in point of time with the prosecutions of Hahnemann by the apothecaries.

But there is no evidence that the change was so very sudden as to require such an explanation. Even so short a period as one year spent in constant practice might be sufficient to satisfy so acute an observer as Hahnemann, that remedies given according to the homœopathic law must be given in very small doses.

The whole career of Hahnemann is a consistent protest against the adoption or renunciation of any procedure connected with the practice of medicine, on grounds of mere policy or expediency. Hahnemann's long life was a continual sacrifice of ease and prosperity and highly prized friendships to his convictions of truth. While therefore, during a period of twenty years he submitted to be driven from city to city rather than renounce the right of dispensing his own medicines, is it at all probable that he devised the method of diluting or potentizing remedies and insisted on the superiority of potentized remedies, simply, or in any degree from motives of expediency?

In subsequent essays Hahnemann advises still more distinctly the administration of potentized remedies, and in the first edition of the *Organon*, published in 1810, he says: "Scarcely any dose of the homœopathically selected remedy can be so small as not to be stronger than the natural disease and not capable of overcoming it." About the same period he advises the use of the ninth dilution of Nux vomica and the eighteenth of Arsenic.

In the earlier volumes and editions of the *Materia Medica Pura*, Hahnemann recommends a particular dose for each remedy. Some remedies are to be given in the first dilution, some in the third, some in the ninth, some in the fifteenth, some in the twenty-fourth, some in the thirtieth.

In subsequent editions of the *Materia Medica* and in the work on *Chronic Diseases*, Hahnemann, as is well known, advises that all remedies be given in a uniform dose—the thirtieth dilution.

In the latter years of his life he speaks of using with great success the sixtieth, one hundred and fiftieth and three hundredth dilutions, and it is well known that during these years he did not confine himself to a uniform dose, but used in some cases the lower potencies, and in some the very highest.

It is not unworthy of remark that as Hahnemann's practical experience in the treatment of disease increased, so did his estimate of the advantage and necessity of using higher dilutions, in at least many cases, likewise increase.

The promulgation of the dynamization theory by Hahnemann, and his adoption of the practice of giving infinitesimal doses, were the occasion of the most violent denunciation of Homœopathy by his professional opponents. Indeed, to this day, this really *subordinate* department of the method is regarded by allopaths as the essential feature of Homœopathy; and to a superficial observer it would seem as if the infinitesimal dose were almost the only obstacle to a blending of Homœopathy and the so-called Physiological School of Medicine.

The progressive advocacy by Hahnemann of the higher dilutions, and especially the introduction by Korsakoff of what are technically known as the "high potencies," (the one hundred to fifteen hundredth) gave rise, as has been said, to a lively and bitter and decidedly personal controversy among homœopathists.

In every party or school which is still a minority, there are always *Conservatives*, who regret the necessity which compelled a separation from the party or school of the majority; who strive to make that separation as little decisive and marked as may be; who stoutly oppose any measure which would tend to increase the division or to make it irremediable; and who hail with delight almost *any* project of compromise or reunion There are also *Radicals*, whose turbulent nature revels in the sensation of opposition; whose tendency it is to lay more stress on points of difference than on points of coincidence; who, having once separated from the majority, will not only entertain no thought of a reunion with them, but consider that fidelity to the principle on which their separation was based, requires that the separating chasm should be made continually wider. They seem to glory, not so much in the *truth* for the sake of which they separated, as in the mere *fact* of separation.

These two classes are necessary in every progressive school. The Radicals furnish the propelling force—the steam. The Conservatives supply the restraining and moderating influence—the fly-wheel.

Among the followers of Hahnemann, the Radicals and the Conservatives were the respective parties to the contest about the potentization theory; and in weighing the arguments which each side brought forward, we must not lose sight of the respective tendencies of these two parties.

The law, *Similia similibus curantur*, which is the essence of Homœopathy, was not an absolute novelty in medicine. Indeed, Hahnemann's first argument in favor of it was drawn from the *records* of medicine. He showed that, in all

ages, many cures had been made in accordance with it. He proved that his contemporaries, often unwittingly and sometimes consciously, applied it in treating the sick. To urge its admission as the *universal law of cure* was, therefore, simply to advocate the recognition, as a universal truth, of that which was already received by medical men as, at least, a truth of partial and limited application.

Accordingly, the opposition to Hahnemann was, at the very first, moderate. It was somewhat of the nature of a demand for more conclusive testimony.

Men of conservative tendencies might very consistently, at this time, have become homœopathists; for it was not unreasonable to anticipate, at some period in the not distant future, a universal recognition of the homœopathic law of cure and a consequent reunion with the dominant power.

But the dynamization theory was in irreconcilable opposition to all the notions of Hahnemann's opponents. It taught that doses inconceivably small were sufficient to effect radical cures in acute and dangerous diseases—that such doses were even more efficacious than larger ones. It taught that instead of the curative power of the drug being directly in proportion to its material quantity, there was reason for believing that, to some extent, at least, the *converse* was true.

There could be no such thing as a compromise on this question. Truth lay either with Hahnemann or with his opponents. It could not lie between them. To maintain this dynamization theory would be to make the separation between homœopathists and the dominant school of medicine perpetual and irremediable

It is easy to perceive what sort of a reception was likely to be accorded to the dynamization theory by the Conservative homœopathists. They could not regard it without unfavorable prepossessions. If compelled at last to admit its soundness, this would not be until irresistible testimony in its favor had been amassed—nor even then without regret

for the fact and deprecation of the obstacles which it created in the way of a reunion with the Old School. All their *a priori* assumptions would be unfavorable to its validity.

The Radicals, on the other hand, would receive with joy this additional element of difference from the Old School of medicine. They would advocate it with enthusiasm. Their *a priori* arguments would lend it a vigorous support.

Thus in relation to a question that can be settled by experiment alone, and by a course of experiment requiring careful unprejudiced observation through many successive years, we have all the conditions requisite for an ardent controversy at the very outset, before any valid evidence could possibly have been gathered by either party.

We shall be less surprised at the vigorous opposition to the dynamization theory, if we recollect the condition of medicine and pharmacy at the beginning of the present century. The posology of that period was decidedly heroic. Drugs were given in the crudest forms. Pharmaceutical chemistry had not yet discovered the essential principles of drugs, the use of which at the present day has made doses of the one-fourth, one-eighth, or one-twentieth of a grain familiar even to practitioners of the Old School. The microscope was still a rude and primitive instrument; quantitative chemical analysis had not attained any great development; much less was there any thought of applying, as is now done, the reactive susceptibility of the living organism to detect, through its specific affinities, particles of matter so small as to elude the microscopist and the chemist. In fact the doctrine of the extreme divisibility of matter rested, at that time, rather on the speculations of the metaphysician than on the demonstration of the naturalist.

For this very reason, on the other hand, the speculations of the metaphysician had at that period the wider scope and the greater authority. And thus it was very easy for some of Hahnemann's more radical followers to erect on a

figurative illustration used by him in the *Organon* a "cloud-capped" theory of the transplantation of the medicinal force from the substance of the drug to the substance of the vehicle used for dilution, and upon the basis of this airy hypothesis to explain all manner of supposed action and reaction of drug-spirit upon diseased force, etc., etc.

These considerations will, I think, justify us in passing over with scarcely a word of comment all that was so copiously written on the subject of the dose, from the date of Korsakoff's first publication to the date of Dr. Trinks' public letter to Dr. Stens in 1859.

The principles involved in the question of the efficacy and advantages of high potencies are these:

1. The *curative power* of a properly selected drug is not in the direct ratio of the *quantity* of the drug.

2. The process of potentization which Hahnemann invented, develops a curative power in substances, which in the crude form, manifest no such power; and it increases the curative power of drugs which in the crude form do manifest a curative power.

Now, although these principles had been satisfactorily established, one would think, by the successful use of the third or sixth potencies, long before the controversy about the "high potencies" began, yet it will be perceived that almost all the objectors who began to write after Korsakoff's publication and Hahnemann's cautious and conditional approval of it, derive their arguments from considerations of the *probable quantity* of the drugs which these high potencies might contain. Thus we are cautioned, even by those who admit that higher potencies have curative powers, that they are not to be trusted in acute diseases which have a rapid course, because, as Dr. Scott expresses it, "in these diseases the vital forces act with exaggerated energy and to act upon them, we require a greater amount of medicinal power, that is, a lower dilution." This statement, it will be perceived, regards the curative power as directly propor-

tioned to the *quantity* of the drug, and begs the whole question at issue.

But when Hahnemann had shown that a more efficient curative power was exerted by the first dilution or trituration, or even by drop doses of the mother tincture of drugs than by massive doses, as he had done to the satisfaction of all homœopathists, long before the higher dilutions were ever made, he had demonstrated that the curative power of drugs is not in the proportion of their material quantity, and had thus established the first principle of the dynamization theory.

When, furthermore, he showed to the satisfaction of all homœopathists, that substances which in their crude state exert no medicinal power, such as gold, charcoal, tin, common salt, etc., etc., do, after dilution, trituration or potentization, come to possess a medicinal power, he demonstrated, in part, the second principle of the dynamization theory.

These demonstrations removing the question of dose entirely from the domain of mechanics, in which *power* is directly proportioned to *quantity*, left the whole question open for *experiment*.

The points to be determined were these:

1. The *curative power* of a drug not being directly proportioned to the quantity of the drug, what relation does exist between this power and quantity? What effect does a continued diminution of quantity exert upon the curative power of the drug?

2. It being admitted that the potentization process does develop the curative power, what are the limits of this development? What varieties in the nature and extent of this developed power are produced by various degrees of potentization?

Obviously these questions were to be solved only by a long course of methodical experiment.

While the controversy on hypothetical grounds was being carried on by the Conservative and Radical homœ-

opathists, reports of cases were accumulating in the homœopathic journals, which furnished indisputable testimony to the action of the higher potencies. Among those who published these records, the names of Gross, Stapf, Hering, von Bœnninghausen, Ægidi and Nunez are conspicuous.

Their testimony was disputed on various grounds. Against some cases it was urged that they were inconclusive, since it was not clearly shown that the cure resulted from the remedy given and not rather from some hygienic prescription or restriction resorted to at the same time. Such criticism is legitimate, and cases to which it is fairly applicable must be excluded.

Against other cases it was urged that the data were not so fully recorded as to enable the reader to make sure of the diagnosis. I do not think such criticism as this is admissible, unless in a very few cases.

The value and authority of clinical records always depend on the reputation of the reporter for knowledge, accuracy and integrity. Now, whether, in reporting a case of alleged pneumonia, the reporter simply state that it was a case of pneumonia, presenting such or such characteristic symptoms, or whether he present a minute and elaborate detail of all the rational and physical signs of the patient, obtaining by the aid of auscultation, percussion, mensuration and chemical and microscopical analysis—in either case, we have nothing to depend upon but the scientific capacity and the integrity of the observer.

If he have these qualities in a sufficient degree to obtain with certainty the requisite physical and rational signs on which to base a trustworthy diagnosis of pneumonia, his *statement* of this diagnosis will be sufficient without the details. If he have them not, no details which he might pretend to give would be deserving of the slightest confidence. These considerations annihilate the objection made by Dr. Watzke to cases published by Gross and Bœnninghausen in the *Archiv.*

In general, however, it were well to exclude, provisionally at least, all cases of doubtful diagnosis.

While cases demonstrating the value and efficacy of the high potencies were accumulating, a few cases were also published in which, the higher potencies having been used without effect, a lower potency or a crude dose of the same drug effected a cure. Dr. Black relates that in a case of headache Lachesis[30] only produced symptoms of nervous disturbance, while Lachesis[6] effected a cure. Dr. Trinks records a case which had been treated by Hahnemann for nearly two years with Rhus in a higher dilution without effect, and which he himself cured within a few months by repeated doses of the mother tincture of Rhus.

The angry controversy was brought to a temporary close by Dr. Trinks' public letter to Dr. Stens, in which he states: "I was born a skeptic. As to the wonder cures (cures by the high potencies) published in the journals, I can only say, I do not believe a word of them." "*Non* credo quia impossibile est," seems to be the creed of Dr. Trinks—the *impossibility* consisting in a mere assumption, on his part, that such preparations *cannot* possess any power. This same argument—"non credo"—may, with equal force, be urged by the allopathists against the first or third dilutions, which Dr. Trinks is in the habit of using.

The argument proves too much. If Dr. Trinks may dismiss with "I don't believe it," the testimony of those who advocate the high potencies, on the same grounds may these reject his accounts above referred to, of the cases in which he alleges that the lower potencies were efficacious where the higher had failed.

But the opponents of the high potencies were not content with seeking to invalidate the testimony of the advocates of these preparations. Some of them presented records of cases in which they had made unsuccessful trials of the high potencies. Of these, many were no doubt made in good faith, and must be accepted as evidence that there

are cases in which the high potencies fail to cure. But the majority of these records are like those of Dr. Watzke, referred to by him, in the *Œsterreichische Zeitschrift*, vol. ii., where he says, the patients on whom he tried the high potencies suffered for most part from diseases in which there was little reason to expect a favorable result from any remedy, in whatever dose, for, he says, they were cases of "dissolving pulmonary tubercle, of fungoid tumor of the brain, of cerebral and pulmonary apoplexy, of spinal paralysis, of chronic hydrocephalus, valvular insufficience of the heart, of fibrous tumor of the uterus, etc., etc.

Yet, no doubt, Dr. Watzke is one of those who would exclaim against the unfairness of Andral in pretending to test Homœopathy in the Paris hospital, by giving a single dose of a homœopathic remedy to a patient laboring under incurable disease!

Dr. Watzke himself, though so bitterly opposed to the advocates of high potencies, is constrained to state in his *resumé* of the proving of Natrum muriaticum, in the fourth volume of the *Austrian Journal*, "I am, alas!—I say, alas! for I would much rather have upheld the larger doses which accord with current views—I am compelled to declare myself for the higher dilutions. The physiological experiments made with Natrum muriaticum, as well as the great majority of the clinical results obtained therewith, speak decisively and distinctly for these preparations." This is strong language from an avowedly unwilling witness!

The conclusion to which an impartial estimate of the evidence concerning the high potencies, up to the year 1850, would lead, is well and justly expressed by a writer in the *British Journal of Homœopathy*, vol. v., p. 154:

"1. The high potencies do act.

"2. They act sometimes very energetically.

"3. They often act curatively with surprising rapidity.

"4. They sometimes cure where the lower dilutions have failed—though the evidence on this point is scanty."

The same writer gives the following points as still requiring to be proved:

"1. That the higher are always to be preferred to the lower dilutions; or that they are generally to be preferred to them.

"2. That they will oftener succeed in curing than the lower."

To these points may be added the question which at that time was still an open question, viz., whether the higher potencies were applicable and trustworthy in acute as well as in chronic diseases.

During the last twelve years a large mass of evidence of a most interesting and conclusive character has been accumulated on this subject. Of so much of this as was furnished by European observers, I proceed now to give a brief summary. Inasmuch as the question has been the subject of somewhat earnest personal controversy among ourselves, and "scars may yet be tender," I prefer to pass by, for the present, with a few trifling exceptions, all that has been contributed by American practitioners. I shall pass over also all endeavors that have been made to explain or to limit by hypothetical reasoning on physiological premises, the action of the high potencies. The question is purely a practical and experimental one.

One assumed explanation, however, of the action of dynamized remedies should be noticed, inasmuch as it will enable us to state distinctly what we mean by a higher potency.

It has been alleged that, admitting that an apparent development of curative power does result from the potentizing process in the case of some drugs at least, such as Silex, Gold, Natrum muriaticum, etc., this is simply because this process renders soluble these substances which, in the crude state, are insoluble, or that it reduces the size of their particles to such an extent that these particles are capable of entering the smallest blood-vessels, and of thus coming into contact with the diseased tissue and of acting directly upon it. This

curative power which is pretended to be developed by potentization is, they say, simply a facility of action produced by minute subdivision.

Hypothetical explanation does not alter facts. If, however, the above were the sole explanation of the action of potentized remedies, then it should result that the highest development of the curative power should be reached in that dilution in which the particles have been reduced to a size appreciably less than that of a blood corpuscle. And, indeed, this ground has been taken by some writers. Now, arithmetical calculation, as well as microscopic observation, fixes upon the third centesimal dilution as that in which the above degree of subdivision has unquestionably been attained. If this view were correct, then, dilutions higher than the third should not display a curative power; or if they do display such power, it should not be in any case superior to that of the third dilution.

Now, if it should chance to result from actual, incontrovertible experiment, that potencies higher than the third do display, with rare exceptions, a greater curative power than the third or lower potencies, this superiority must be directly attributable to the process of potentization.

I deem it right, therefore, to call all those potencies above the grade of that of which the action admits a mechanical explanation—"the *higher potencies*." The term will apply, in this view, to all above the third centesimal.

In 1850 Drs. Wurmb and Kasper took charge of the Leopoldstadt Hospital, in Vienna. Their views on the subject of the dose were no secret. They had no faith whatever in the higher potencies. They were physicians of much more than ordinary scientific attainments, and of great devotion; and Dr. Wurmb, at least, had few superiors in an exact and thorough knowledge of materia medica. In their "Clinical Studies," published in 1852, they thus express the posological views with which they entered on the charge of the Hospital:



"We have given almost always the *thirtieth decimal* dilution, and only exceptionally a higher or a lower dilution. * * We propose to adhere to this dilution for two years longer, then to give another dilution for an equally long period, and finally to give for a similar period still another dilution. Such experiments as these are indispensable to the solution of the question of the dose, but manifestly they are valid only in the case of disease with regard to which the preliminary question, What can Nature do, and what can Art? has already been definitely answered, and in favor of the latter?"

Here is the plan of an experiment which, if faithfully carried out, gives promise of some very conclusive data on the subject of the dose. Such questions as this require for their solution a multitude of instances such as can hardly be gathered in private practice. But a hospital affords an appropriate and sufficient field for their collection. The hospital of Dr. Wurmb is the only one which has been devoted to such uses.

Before the expiration of the three periods, of three years each, involved in the plan as above stated, Dr. Kasper was succeeded by Dr Martin Eidherr, who has published in the *Œsterreichische Zeitschrift*, for 1862, the result of the ten years' experiment.

It seems that in 1860 the Austrian Homœopathic Society of Vienna proposed as a subject for consideration the question of the dose, inviting all homœopathists of all countries to take part in the discussion. A large number of responses, both verbal and written, were made to this invitation, some of which are published in the *Austrian Journal*, for 1862. Dr. Eidherr, the Editor of the Journal, remarks: "These discussions have, as is well known, led as yet to no positive result, because they rest exclusively on subjective grounds. While some record most brilliant cures effected only by the higher dilutions, others narrate similar cases cured only with the lower. Consequently, the society regards the question

as still an open one and calls for further communications on the subject."

At this point it was suggested that the material in the shape of clinical records which had been for ten years accumulating in the archives of the Leopoldstadt Hospital might, if collated, throw some further light on the subject.

Dr Eidherr undertook the task. He resolved to confine his investigations to a single disease, pneumonia (which by the way is very prevalent in Vienna), for the reason that the diagnosis of this disease is easy, and that, by means of the physical signs, its course and progress and decline may be more accurately followed and observed than is the case with many other acute diseases.

During the ten years, from 1850 to 1859 inclusive, all cases in the Leopoldstadt Hospital had been treated for the first three years with the thirtieth decimal dilution, for the second period of three years with the sixth, and for the remaining four years with the fifteenth decimal dilution. It was proposed to compare the results of the treatment of pneumonia during these three periods

But, in order to avoid a fallacy in drawing conclusions from this comparison, it was necessary, first, to inquire whether the Genius Epidemicus was the same for these three periods, or, if not the same, how great an influence, and in favor of which period, did the difference exert?

To meet this preliminary question, Dr. Eidherr made a study, first, of those conditions which favor the origin and spread of pneumonia: and, second, of the prevalence and course of pneumonia in the great General Hospital of Vienna during the same periods of time.

The first section of his treatise consists of tabular statements of the meteorological phenomena of the decennium in question, and of the relations of these phenomena to the prevalence of pneumonia as observed in the great General Hospital of Vienna.

The second section comprises short and succinct accounts

of the cases of pneumonia treated in the Leopoldstadt Hospital during the three periods into which, as already stated, the decennium was divided.

The third section contains a statement of the results of the treatment by the different dilutions used during the three periods in question, taking into account the modifying influence of the different atmospheric conditions of these periods.

The cases occurring during the three periods of time into which the whole period of ten years was divided, are called by Dr. Eidherr—Groups 1, 2, 3.

Group No. 1, embracing the years 1850, 1851 and 1852, was treated exclusively with the thirtieth decimal potency.

Group No. 2, embracing the years 1853, 1854 and 1855, was treated exclusively with the sixth decimal potency.

Group No. 3, embracing the years 1856, 1857, 1858 and 1859, was treated exclusively with the fifteenth decimal potency.

A careful estimate of the different meteorological conditions of these epochs leads Dr. Eidherr to the conclusion that during the first epoch the atmospheric conditions were most favorable to the prevalence and severity of pneumonia, and therefore the *least* favorable for the treatment; during the *second* epoch *least* favorable for the spread and severity of pneumonia, and therefore the *most* favorable for the treatment.

In observing and recording cases of pneumonia in this hospital, the physical signs have always been carefully noted. and records have been made of the following points in the history of each case:

1. The seat of the infiltration.

2. Its duration, reckoned from the time at which it was first perceived to the time at which it was noticed that it began to be resolved (?).

3. The time at which resolution of the infiltration began.

4. The time at which resolution was completed.

5. The time at which all physical signs disappeared.

6. Duration of convalescence.

The comparison between the three groups is made with reference to all of these points.

The results are stated as follows:

Group 1, treated with the thirtieth decimal dilution;

The average duration of the infiltration was three days.

The average date of commencing resolution was the third day.

The average date of completed resolution was 4.9 days from the beginning.

The average date of vanishing of the physical signs was, as concerns the infiltration, 7.1 days; as concerns the exudation, 12.3 days from the beginning.

The average duration of convalescence was 4.4 days.

Group 2, treated with the sixth decimal dilution during the years 1853, 1854 and 1855;

The average duration of the infiltration was 4.1 days.

Resolution began, on the average, in 3.5 days, and was complete in 6.9 days.

The physical signs disappeared, on the average, as concerns the infiltration, in 9.3 days; as concerns the exudation, in 20.5 days.

The duration of the convalescence was, on the average, 5.3 days.

Group 3, treated with the fifteenth decimal dilution, during the years 1856, 1857, 1858 and 1859;

The average duration of infiltration was 3.4 days.

Resolution began in 3.2 days.

It was complete in 6.3 days.

The physical signs disappeared, so far as the infiltration was concerned, in 10.3 days; so far as the exudation was concerned, in 18.1 days.

Average duration of convalescence, 4.8 days.

To recapitulate the above—

The average duration of the infiltration was:

For Group	1,	3.0 days.
"	2,	4.1 "
"	3,	3.9 "

Resolution began:

For Group	1,	on the	3d day.
"	2,	"	3.5 "
"	3,	"	3.2 "

Resolution was complete:

For Group	1,	on the	4.9 day.
"	2,	"	6.9 "
"	3,	"	6.3 "

The physical signs of the infiltration vanished

For Group	1,	on the	7.1 day.
"	2,	"	9.3 "
"	3,	"	10.3 "

The physical signs of the exudation vanished

For Group	1,	on the	12.3 day.
"	2,	"	20.5 "
"	3,	"	18.1 "

Dr. Eidherr gives also a tabular statement of the average number of days during which each case of each group remained in hospital—that is, the total duration of each case from its reception to its dismissal, as follows:

Group 1, treated with the thirtieth decimal dilution; fifty-five cases were treated; their aggregate residence in the hospital amounted to 680 days; or an average of 11.3 days each.

Group 2, under the sixth decimal dilution; thirty-one cases, 606 days; an average of 19.5 days for each case.

Group 3, treated with the fifteenth decimal dilution; fifty-four cases, and 795 days; an average of 14.6 days for each case.

I now proceed to give as briefly as possible the conclusion to which Dr. Eidherr is led by this careful study of his statistics. He says: "This is the most extensive experiment that has ever been made, bearing on the question of the dose. Its subjects were 107 cases of pneumonia. Each case was the subject of careful investigation. Every imaginable care was taken to obviate every source of fallacy." The experimenters were not radical homœopathists. Their prepossessions were rather against the high potencies. I can bear personal testimony to the fact that, in 1851, while the thirtieth dilution was the standard used in the hospital, Dr. Wurmb frequently expressed himself as believing that statistics would decide in favor of lower dilutions. It was not known how statistics had decided until Dr. Eidherr made the analysis from which I have quoted, and which shows that in every point of view the action of the thirtieth dilution, in so acute and dangerous a disease as pneumonia, is more certain and more rapid than that of the fifteenth or the sixth dilution, and that the fifteenth is preferable to the sixth dilution—or, to translate the decimal into the centesimal scale, the fifteenth is better than the seventh, the seventh than the third.

Against this record Dr. Trinks would raise in vain his cry "*non credo.*"

If we refer now to the points which were stated in 1850 as still requiring to be proved in relation to the high potencies, we find the third question satisfactorily solved by this experiment in the Leopoldstadt Hospital. For the experiment proves beyond a doubt, that the higher potencies are applicable and trustworthy in *acute* as well as in *chronic* diseases.

It enables us also to give a probable affirmative answer to the first question, and to say that the *higher* are, at least so far as pneumonia is concerned, to be *preferred* to the *lower* dilutions.

The second question, viz., whether higher dilutions will

oftener succeed in curing than lower dilutions, is left unaffected by this great experiment.

Let us now recapitulate the points at issue. I think we may safely state that, up to the present time, experience has established these facts: that the continued diminution of the material quantity of a drug through the process of potentization does not diminish the curative power of the drug when homœopathically applied; that, on the contrary, the process of potentization does positively increase the curative power of a drug when homœopathically used; that this increase of curative power is progressive, as far at least as the fifteenth centesimal dilution, and is demonstrated in the treatment of both acute and chronic diseases.

It was incumbent on the advocates of the higher potencies to demonstrate not only that these preparations are efficacious, nor simply that they are equally efficacious with the lower potencies, but that they are *superior* in efficacy.

This has been shown by the Vienna experiment up to a certain degree. The seventh centesimal has been shown to be superior in the treatment of pneumonia to the third, and the fifteenth to the seventh. There is a natural desire to find the limit of potentization. The Vienna experiment gives us no reason to suppose that this limit has been attained in the fifteenth potency.

Many most eminent practitioners assert for what are technically termed the "high potencies," as great a superiority over the fifteenth and thirtieth as the latter have been shown to possess over the third. Cases are accumulating in our journals which go to corroborate these assertions. We cannot refuse to believe these narrations. While they have not yet established a general superiority of the "high potencies" (sixtieth to two hundredth) over the fifteenth and thirtieth for the treatment of all cases, acute and chronic, they nevertheless prove beyond dispute that the rapid and permanent cure of both acute and chronic diseases is within their scope.

They establish, moreover, the fact that in many cases these high potencies (the two hundredth, for example) cure diseases in which the lower potencies of the same drug have failed.

The determination of absolute superiority and of definite limits of increase of curative power, is impossible from our present data.

Among those who have long used the high potencies (in particular the two hundredth), Dr. von Bœnninghausen stands pre-eminent. It is true that his practice lies chiefly among chronic diseases. Nevertheless, many acute diseases are treated solely in accordance with his directions. He has for many years used exclusively the two hundredth potencies. His increasing fame and practice attest the validity of the services which he renders to those who consult him. The high character which he has always borne as an acute and sagacious observer of natural phenomena, the unblemished reputation which he has sustained throughout his long and eventful life, cause his observations and his statements to be received with implicit reliance and belief by all who are at all cognizant of his social position. He does not hesitate to express a decided conviction of the great superiority of the high potencies over the lower, in both acute and chronic diseases.

Dr. Ægidi, who, after having, on their first introduction, spoken favorably of the high potencies, retracted to some extent this good opinion, has recently announced as the result of a long course of experiment in his private practice, embracing the history of more than 4,000 cases, a clear and unequivocal preference for the high potencies in both acute and chronic diseases.

Dr. Battman has, within the last two years, published a series of cases of severe membranous croup, and of very dangerous acute pulmonary affections, cured in a marvelous manner with the two hundredth potency; and basing himself on these narrations, he makes a warm protest against

the irrational conduct of those homœopathists who, at the same time that they object to the illogical conduct of the allopathists in refusing even to listen to any evidence in favor of Homœopathy, obstinately close their own ears to all that is alleged in behalf of the high potencies.

Of six physicians of Austria and Hungary who presented written essays on the question of the dose to the Vienna society, none denied the curative action, often prompt and surprising, of the high potencies (one hundredth to the three hundredth) in both acute and chronic diseases. Only one expressed a doubt that they were in any case superior to the lower potencies. Two did not hesitate to claim such superiority for them in decided terms.

Dr. Wurmb, the head of the Leopoldstadt Hospital, states[1] that often remedies in the higher dilutions act, where the lower dilutions of the same remedy had failed to act. He gives an instance in which Belladonna^{100} effected a cure in chronic migraine, in which a lower potency of the same remedy had failed to do any good. The one hundredth and two hundredth potencies are now not infrequently administered in the Leopoldstadt Hospital. Dr. Wurmb remarks[2] that "since he has had experience with the one hundredth dilution prepared by Dr. Eidherr, and has satisfied himself of the wonderful effects which often result from them, he prescribes them in *suitable* cases with greater preference than the thirtieth or any other potency." He does not state what he considers to be "suitable cases." It is probable that he does not consider his experience to be great enough, as yet, to justify him in laying down any general rules on the subject. It is likely that he decides for each particular case, according to his view of *probabilities*, and that he cannot always give a clear reason for his preference of one potency over another. Each case thus treated may be regarded as in some sort an experiment—a contribution to that multitude of instances from an analysis of which we

[1] *Œst., Zeit.*, I. 158, 1862. [2] Loc. cit. 3, p. 137.

may hope to deduce, at some future time, a general law of *posology*.

If, after this general view of the evidence on the subject of the high potencies, it be appropriate for me, on such an occasion as the present, to state my own experience and methods, it may be done very briefly.

Before I had fairly entered to any great extent upon the responsibilities of the practice of medicine, I had the advantage of observing the practice of some very eminent physicians and of listening to the counsel of others.

Not to mention American physicians, I heard in England chiefly the advocates of the low and the lowest potencies. The high potencies were at that time rarely mentioned in England except in terms of ridicule. And the contempt which the assumed non-material nature of these potencies inspired in the matter-of-fact mind of that taurine nationality, whose devotion to the pound avoirdupois is even greater, if that were possible, than to the pound sterling, was cordially extended to the intellectual acuteness and the scientific acquirements of all who believed in these potencies and who used them. Since that period the cause of the high potencies has been nobly advocated in England, by a few of those strong men, who are the true glory of the nation.

Passing from England to Westphalia, I enjoyed free and full and long-continued opportunities to observe the practice of Dr. von Bœnninghausen, who used the two hundredth potency exclusively. There was, certainly, in his practice, less parade of scientific auxiliaries than I had seen at the English hospitals or in the English dispensaries; but I do not think the diagnosis was less accurate on that account. I am very sure that the success of the treatment left but little to be desired. Though some acute diseases were here treated with high potencies under my observation, yet the majority were chronic cases, and, deeply impressed with the great responsibility involved in my judgments, I was hardly ready, from these observations, to conclude on the advantage of

using high potencies in a *general* practice. Nevertheless, the effect of these observations was such as to satisfy me of the efficacy of the higher potencies in all forms of disçases. The question remained, "Are they *superior* to the lower?" Stapf, who was then living in retirement, counseled me to use the high potencies in chronic diseases, and thought the medium potencies were all that could be desired in acute diseases. He had no statistics of comparative observations by which to justify his opinions.

In Vienna, Wurmb and Kaspar were in the middle of the first epoch of the decennium of which Eidherr has given us the analysis. They were treating all diseases with the thirtieth decimal dilution. Their examinations of patients were minute and masterly, their prescriptions careful, their success very striking.

Fleischmann, at the same time, was giving the mother tincture or the lowest dilutions. His diagnosis was careless, his success neither rapid nor, except as compared with heroic allopathic treatment, very striking. His prescriptions were not accurate. He did not individualize his cases, but prescribed according to a coarse and rude generalization. On the subject of the potency, his opinions, I learned from himself, were immovably made up. I perceived that he had ceased to be capable of learning, although he still lived; that, in knowledge, as in stature, he had attained his growth; ossification was complete in his perceptive intellect no less than in his physical skeleton.

In Paris, Tessier was treating acute diseases in the hospital Ste Marguerite with the sixth, twelfth and fifteenth dilutions. His success exceeded that of Fleischmann. It was inferior to that of Wurmb, but it was easy to perceive how inferior he was to that excellent physician in minute and comparative knowledge of the materia medica.

In Paris, Dr. Perry was prescribing at his dispensary the two hundredth potency, with but few exceptions, in all forms of disease. The reports indicated a very wonderful success,

but, from the nature of a dispensary practice, this could not be made the subject of exact observation.

My conclusions from all these observations were favorable to the high potencies, so far as to admit their energetic action on acute as well as chronic diseases and their *general* superiority in the treatment of the latter; but I was not satisfied that they were so trustworthy, as a general rule, in the treatment of acute diseases as to be preferable to the lower potencies. I therefore began, in my own practice, with the use of the lower and medium dilutions (third to twelfth and fifteenth) in acute diseases. When very sure that my selection of the remedy was entirely correct and that the case would not suffer should my first prescription prove inert, I ventured, during the first few years of my practice, to give a high potency in acute diseases I now look back on this period with wonder that the idea of a definite and direct relation between the curative power and the material quantity of a drug should have been so hard to eradicate from my mind.

Experience of the action of the higher potencies, on my own person, in very acute illness, first fully convinced me—perhaps, because, in my own case I took the risk of trying the experiment more freely and fully than I had ever done in the case of others.

For the last five years[1] I have used the high potencies (two hundredth of my own manufacture and of Lehrmann's, indiscriminately) in all forms of disease that occur in a general practice. I am very sure that my practice has grown more successful every year. While I trust that an increasing knowledge of the Materia Medica may have contributed greatly to this result, I cannot be mistaken in the belief that much is also due to my more and more frequent use of the high potencies.

I cannot say that every prescription has done everything I expected or hoped from it. Nobody's practice can be

[1] Written in 1863.

free from painful failures. In such cases, wherever I have been tolerably sure that my choice of remedy was correct, I have repeated the same remedy in a higher or lower potency as the case might be, and while I have collected from my practice many instances in which a high potency has acted promptly where a low had failed to act, I have noted only one case in which a high potency acted but inefficiently while a lower gave prompt and complete relief.

From my own experience, then, the presumption would lie in favor of the high potencies in both acute and chronic diseases.

A few instances will serve to illustrate what has been said. They are presented only as illustrations, by no means as the evidence on which my convictions rest.

A gentleman who had suffered many years from necrosis of the femur was subject to acute attacks of periostitis. The twelfth potency of Asafœtida was found to relieve his suffering, and repeated doses of it generally effected a cure within three or four days. After treating several attacks in this way, I gave him, at the commencement of a fresh attack, a dose of Asafœtida^{200}. The cure was effected in the space of six hours. A difference so remarkable was very obvious to the patient, who, learning from me the difference between this and my former prescriptions, requested to be treated always thereafter with the two hundredth. Here, whatever cavils may be raised about the diagnosis or other points, the fact remains incontestable, that attacks which required for their cure several days and repeated doses of Asafœtida^{12}, were cured in six hours by a single dose of the two hundredth. Could it be that repeated treatment of these attacks had modified their severity, and that the amelioration chanced to coincide, in point of time, with the change of potency? To satisfy myself on this point, I once reversed the experiment, and without my patient's knowledge gave the twelfth instead of the two hundredth potency. The attack came on with its ancient severity and persistence,

much to my patient's disgust, who was abundantly satisfied with the high potency, but much ashamed of *my* want of confidence.

Even after I had become quite satisfied of the superiority of high potencies in most acute diseases, I yet hesitated to employ them in a malady so fearful and so rapid as croup. In this I still adhered to the low (the third) dilutions of Aconite, or Spongia, or Hepar, as the case might require, or to the watery (first centesimal) dilution of Bromium or Iodine, if these remedies were indicated. Notwithstanding I had actually witnessed most surprising success in Dr. von Bœnninghausen's practice with the two hundredth potencies in severe croup, I hesitated to use them. I argued to myself: "These low potencies have served me well. The majority use them. I do not know that the high are better, even if they be as good. The success with them may be exceptional. I dare not risk the loss of time which would accrue from an unsuccessful experiment with them."

Thus it turned out that I never used the high potencies until, three years ago, in the most severe case of membranous croup I ever saw, the low potencies in which I had always trusted failed me utterly, and I knew not what else to do. A resort to the use of the two hundredth potencies of Aconite, Hepar sulphuris and Spongia saved my patient from this extremity of danger, and satisfied me that a trial of the high potencies in the outset of an attack of croup, instead of involving a risk of wasting time, does in truth obviate such a risk from the employment of the lower potencies. From this time on, in the treatment of croup, I have uniformly begun with the two hundredth potency of whatever remedy was indicated. My success has been more uniform and much more rapidly attained than ever before.

My first use of a high potency of Bromine was accidental. Called to prescribe for a severe case of croup, in which that remedy was indicated, I found that the crude substance or a low dilution was not to be obtained. I had the two

hundredth potency in my pocket-case. I gave it with a result equally happy, and much more speedy than I had ever before witnessed. This was altogether contrary to my preconceived notions concerning Bromine, and it summarily upset a very pretty chemical theory I had formed.

As already remarked, I have sometimes met with facts of a contrary significance. In a case of chronic asthma, of great severity, I have recently found Glonoine of the greatest service. I first prescribed the sixth potency, having no other at hand. When the action of this dose was exhausted I gave the two hundredth. The result was by no means satisfactory. The sixth again produced happy effects as before. Repeated experiments of this kind convinced me that in this instance the high potency did *not* act so favorably or so efficiently as the low. Whether this peculiarity should be accounted for by assuming an idiosyncrasy on the part of the patient, or a peculiarity of Glonoine which renders it incapable of high potentization, or whether the action of the Glonoine in this case will prove to have been only palliative, and therefore temporary, is a question which can only be solved by a wider experience than I possess in the use of various potencies of this new but valuable remedy.[1]

But this fact that a low potency succeeded where a higher had failed, together with similar facts reported by other practitioners, must have a bearing upon general conclusions.

In conclusion, I think the following statements are warranted:

1. In prescribing, the first essential is the correct choice of the remedy. The second point—which is also, in many cases, though *not always, essential*—is the judicious choice of the *potency*.

[1] The subsequent history of this case confirms the suspicion that the great relief afforded by Glonoine was palliative. After a few months the disease recurred with its original severity, and no form of Glonoine (nor of any other remedy that I tried) availed to give relief.

2. That, in both acute and chronic diseases, the preference, other things being equal, is to be given to the higher over the lower potencies. The experience of Wurmb shows that, in severe pneumonia, the fifteenth is to be preferred to the seventh and the seventh to the third (centesimal). I should go further and say that, in general, preference should be given to still higher potencies, even to the two hundredth.

3. Experience shows that, while the majority of cases, both acute and chronic, are cured more speedily by the high than by the lower potencies, yet, in some cases, the converse is observed. No explanation of this difference has been discovered, nor can its occurrence be foreseen in any case. While therefore the presumption, in every case, being in favor of the high potencies, the treatment should be begun with them, nevertheless, should no favorable result ensue, recourse should be then had to lower potencies, provided always there be a reasonable certainty that the remedy has been rightly selected.

4. The question, whether the "high potencies" are more generally successful than the lower, and in what proportion they are so, is yet to be determined by statistics drawn from methodical experiment.

5. A general law for the *a priori* selection of the potency suitable for a concrete case, if such a law be possible, is yet to be discovered

REPLY TO A LETTER ON HIGH POTENCIES.

The questions contained in the following letter, recently received, represent a class of queries that are frequently propounded both orally and in correspondence. It has appeared not inappropriate to publish a general reply to these and similar questions:

"1. In what form do you use these potencies, in the shape of pellets or liquids?

"2. Do you use the *decimal* or the *centesimal* scale in the preparation of the high potencies?

"3. Do you repeat the high potencies in rapid succession in acute diseases, as you do the *low* preparations?

"4. If you use pellets, do you consider them perfectly reliable? and how many, as a general rule, constitute a dose, either dry or taken in water?

"5. Do you alternate the high potencies, or do you rely upon the single remedy?

"6. Do you believe that the high potencies, from your own experience in the use of them and from what you have beheld in the practice of others, are far superior, in all respects, to the low preparations as remedial agents?

"Some of these questions may seem in themselves to be insignificant, but I do not consider them so, for in making my first trial I wish to *start right*, that the experiment may be made fairly, with a sincere desire to know the truth, the whole truth, and nothing but the truth.

"In conclusion, let me say that you may rest assured your answers to this communication will be kept perfectly private, and no publicity (as coming from you) given them.

"Permit me to repeat that it is only a sincere desire to

seek out and find the truth that has led me thus to trouble you"

We heartily respect our correspondent's earnest desire to get at the "whole truth," and to "start fair" in his experiment; and we respect his hesitation to use the high potencies until he should have a reasonable assurance that in so doing he would not be hazarding the interests of his patients. We take this public manner of replying to his questions, partly with a view of convincing him that we have no *wish* that our "answers to this communication" should be "kept strictly private" On the contrary, we should be glad to have "publicity (as coming from us) given them" For they are expressions of our honest convictions, the result of as careful observations and as cautious and complete experiments as we have up to the present time been capable of making. They express our present opinion,—those views in accordance with which we shape our daily practice.

But we hold ourselves bound by them only so long as they shall continue to be our honest convictions. Should further observation and more extended experience satisfy us that any of our present positions are untenable we shall gladly abandon them for others, and shall then, likewise, be not only willing but anxious to have "publicity (as coming from us) given" to these new views The object of our professional life is to find out the truth and to shape our practice accordingly. Consistency to this object is *true* consistency; while consistency to any form of opinion or doctrine that may at one time have been supposed to be the truth and proclaimed by us as such,—consistency to such opinion merely because we may have once publicly uttered it, — this is the basest and most ignoble bigotry and cowardice.

In the article[1] to which our correspondent refers, we had no object but to express frankly and plainly the views which govern us in the practice of medicine. We would call his attention to the fact that the greater part of the article

[1] "The Use of High Potencies in the Treatment of the Sick."

consisted of citations of the opinions and experiences of other practitioners whose conclusions were but corroborated by our own.

1. *In what form do you use these potencies, in the shape of pellets or of liquids?*

As a matter of fact, we use them in the *form of pellets.* As a matter of faith, we know no difference between pellets, pills, triturations or liquids. The pellet is merely a convenient means for dividing a drop of liquid into a number of equal parts, and it is for this object that we use them. We have been in the habit of buying unmedicated pellets or globules at Smith's Pharmacy, and medicating them with liquid potencies of our own preparation. Pellets thus medicated we find retain their remedial powers for several years *at least,*—exactly how long we cannot say. They are so much more portable and more convenient to administer than liquids, that this furnishes us a sufficient reason for preferring them to the liquid form of prescriptions. A writer in the *British Journal of Homœopathy,* some years ago, published an essay on the Globule versus the Pillule, making out a very bad case for the unlucky globule as calculated to bring Homœopathy into contempt in the eyes of persons not indoctrinated. There is something laughable, if it were not deplorable, in this argument, the pith of which is this: "We give small doses, to be sure, but let us not, by using the pellet, *appear* to give small ones; let us use as big a pill as anybody that we may not *seem* to give a small dose, and may not rudely jostle the prejudices of our patients." But surely, if contempt would have damaged Homœopathy, this luckless science, despised, scorned, ridiculed, and, scores of times, extinguished by Homeric laughter, should have been done for long ago! To adopt and defend this much contemned science, and yet to shrink from the obvious smallness of a pellet-dose—is not this "to strain at a gnat and swallow a camel."

Nothing will gain the confidence of a patient so surely as

success! His confidence, once gained by *success*, cannot be shaken by the form of your dose! Yes, it may though! If he sees that while your doctrines require you to give small doses, you yet dissemble and juggle, and, by using large pills and lozenges and mixtures, try to make it appear that you are giving as large doses as your Old-School neighbor, he will suspect that your faith in the system you profess is not really strong, and he will have doubts of both you and your system. The sick man who feels that you are *curing* him, cares not a straw for the logical improbabilities of your doctrines, nor for the scientific difficulties attending the explanation of the action of your little dose. Large or small,—much or nothing,—if, under your auspices, his health return, he will have faith equally in yourself and in your methods.

We have been amazed at much that has been said and written on this subject. Our own patients have rarely remarked upon the pellet. One, who had never seen them before, once said to us, "Do you really hope to cure me with those tiny pills?" "Yes, certainly." "I should not believe they could possibly have any power." "Why? Because they are so fine and small for pills?" "Yes." "Then, my dear sir, instead of regarding them as '*fine pills*,' consider them to be very *coarse* granular *powder*, and you cannot fail to be impressed, *a priori*, with their immense power!" He perceived the absurdity of his objection, which was to the outward form and not to the inherent power. The *success of the prescription* satisfied him of the virtue of little pills.

Another patient objected to the very small vials of my pocket-case. I replied they were a matter of convenience to me, but if he would be better satisfied I would, next day, bring his dose in a quart bottle and pour out the same quantity (pellets). He also perceived that his objection was frivolous, and was content.

2. *Do you use the decimal or centesimal scale in the preparation of the high potencies?*

As a *matter of fact*, we use the *centesimal* scale in preparing the high potencies and all the potencies which we use, and have used since we began to practice medicine. As a matter of opinion we see no reason to prefer the decimal. It does not insure a more uniform gradation as has been claimed. The use of it leads to confusion and is to be regretted. We prefer adhering to Hahnemann's scale. It is easy to convert the one into the other in reporting cases or in reading reports. If our correspondent will refer to the article which prompted his letter, he will note that in the treatment of pneumonia Wurmb and Eidherr used potencies prepared on the decimal scale, which fact we there stated and we reduced their numbers to the corresponding ones of the centesimal scale. In general in this country where the facts are not specifically stated, it is understood that the centesimal or Hahnemann scale is intended.

Our own preparations were made in strict accordance with Hahnemann's directions and so are the high potencies of Lehrmann, as we have learned from Dr. von Bœnninghausen, who directed their preparation, and from Lehrmann, himself.

3. *Do you repeat the high potencies in rapid succession in acute diseases, as you do the low preparations?*

How shall such a knotty question be unraveled? It involves two assumptions, two beggings of the question, viz.: 1. That we do repeat the low potencies in rapid succession in acute diseases; and, second, that we make any such distinction between acute and chronic diseases, as to admit of a radical difference in our principle of prescribing.

1. We recognize but one rule touching the repetition of the dose. It was laid down by Hahnemann and is as follows: Do not repeat the dose of the remedy given until the effects of the previous dose shall have ceased to be evident. Our most grievous failures have come from a violation of this rule. Our most brilliant and complete successes have coincided with a strict observance of it. If we are *sure* that our remedy has been rightly selected, we

sometimes direct, particularly in cases that have been actively treated by allopathic physicians before we were called, and in which we apprehend a sluggish response to remedies, a repetition of the dose every few hours, until some amelioration or decided aggravation appear, but we always order a suspension of the remedy as soon as *either* is *manifest*.

2 We know of no clear distinction between acute and chronic diseases on which to base a difference in treatment. Indeed no difference whatever, unless it be one analogous to that which Hahnemann laid down, viz.: that chronic diseases are based on the awakening of miasms that had hitherto lain dormant in the system.

For instance, is scarlatina an acute disease? Assuredly it is so regarded. Yet, on the third day, scarlatina often shows that it has awakened and ingrafted itself upon the scrofulous (or *psoric?*) taint in the patient's constitution and then, surely, it becomes typically chronic. This is an example of what we see happen in all forms of disease. It prevents an available distinction between acute and chronic diseases.

We can assure our correspondent that it is safe and advantageous to strictly follow the Hahnemannian rule about the repetition of the dose in acute no less than in chronic diseases. But let us anticipate a possible confusion in his mind. Some writers—Dr. Drysdale we are surprised to see among them—seem to think that Hahnemann, when he said, "Wait till the first dose shall have exhausted its action," meant to say, "Do not repeat the dose until that period shall have elapsed which I have indicated in the *Materia Medica* as the duration of action of each drug." This period for some drugs is several days, for others several weeks or even months. We do not so understand him. The duration of action of a remedy on the healthy subject (prover) furnishes no criterion of the duration of its action on the sick. Again, the duration of its action on one sick person furnishes no criterion of the duration of its action on another sick person. Surely the vital processes are much

more rapid in acute pneumonia than they are in tuberculosis. Is it not probable that the duration of action of a dose of medicine would be shorter in the former than in the latter?

We suppose Hahnemann meant as follows: "If amelioration follows a dose of medicine, do not repeat the dose until the amelioration ceases to progress, *Then*, if the symptoms be the same as before, though mitigated in severity, repeat the dose. If the symptoms be different, study the case anew and make another selection of remedy." It is in this sense that we have understood and that we apply Hahnemann's rule. Not pretending that we do not often, through errors of judgment, infringe it, we are sure that whenever we do so, misfortune follows, and that in proportion to our faithfulness, so is our success.

In respect of the repetition of doses, as well as the form of the prescription, we have no difficulty with our patients. Patients are like soldiers; they believe in a man who believes in himself. We say this in all humility, for, in a matter of science, belief in one's self is faith in the laws one has undertaken to carry out in practice. And if the physician show confidence in his methods, his patients will yield themselves implicitly to his guidance. The prejudice in favor of large and many doses is a relic of past ages, when the practitioner was paid, not for his skill and personal services, but for the *medicines* he furnished,—a barbarous usage, which, along with slavery, we received from our British progenitors. Unlike them, we have discarded the former but not the latter.

4. *If you use pellets, do you consider them perfectly reliable?* Assuredly, or else we would not use them. We medicate them ourselves.

And, *how many constitute a dose?* If, properly medicated, one is as good as *one hundred*. As there is a possibility that, in medicating several thousands at one operation, a pellet here and there may fail to get saturated, we usually

give about four to six. We use the smallest pellets as most easily and surely medicated.

5. *Do you alternate the high potencies, or do you rely upon the single remedy?*

Here again our friend confounds a *principle* and *quantity*. If it be right and advantageous to alternate the low, it is right and advantageous to alternate the high potencies. But, in fact, we do not alternate at all. We *always* rely on the single remedy at one time. Dr. Drysdale says that *everybody* alternates, and, therefore, there must be some necessity for the practice. But his illustrations are so far fetched, and his definition of alternation is so contrary to the conceptions which all other homœopathicians, from Hahnemann down, have had on the subject, that, notwithstanding our respect for Dr. Drysdale, we must repeat, in the very face of his learned paper, *that we do not alternate*.

Our understanding of the practice of alternation, and our objections to it were stated, as well as we are able to state them, in the *American Homœopathic Review*, June, 1863, vol. iii., No. 12.[1]

We are opposed to it in theory, and we abjure it in practice. It is an abominable heresy. As a shot-gun maims where the rifle would kill, so alternation may change and modify and maim the disease, but it never does nor can effect the clean, direct and perfect cure that a single remedy, exactly homœopathic, will accomplish. As a relic of the polypharmacy which has been the stumbling-block of the Old School, we loathe it. As a refuge of the careless prescriber and slothful student, we despise it. As an anomaly in homœopathic practice, a fatal obstacle to progress in the clinical portion of our Materia Medica, we deplore it.

6. *Do you believe that the high potencies, from your own experience in the use of them, and from what you have beheld in the practice of others, are far superior in all respects to the low preparations as remedial agents?*

[1] See "Alternation of Remedies," in this book.

An affirmative answer is involved in the statement that we use the high in preference to the low preparations. For details we refer again to the article which prompted our correspondent's letter. Personally we have suffered, and do now suffer, from chronic organic disease, and from occasional very violent acute attacks. We always use the high potencies in these cases, preferring them to the low. We use them in our family and among our friends. We use them in general practice. Many of our friends and patients, non-professional persons, know the fact and freely say that they and their children are more speedily cured by the high than by the low potencies.

N. B. The imaginations of our friends aforesaid are not more lively than those of the average of other people. Indeed, they are plain, matter-of-fact persons, possessing much common sense, but, for the most part, no genius. They prefer high potencies.

And now, having replied in detail to the queries of our correspondent, let us add a few remarks which we beg him to receive in good part.

To change the aspect of a case, to cause the original symptoms to be supplanted by other symptoms, this is no *more a cure* than "*a strategic change of base*" is a "victorious campaign." Yet this may be effected by repeated doses of a drug in a low potency, whether the drug be strictly homœopathic to the case or not. And a succession of such changes and supplantings may be effected, day after day, until finally the patient gets well or nearly so. Meanwhile the patient may be amused by the varieties which each day brings forth, and if he know nothing of a true homœopathic cure, but have heretofore had only the heroic treatment, he may fancy he has been doing finely.

Now, in this way, with low potencies, a practitioner may do quite a business on a very slender capital of knowledge.

Not so if he use the high potencies. With these no change is effected in the case unless the remedy have been

strictly homœopathic to the case. They are like the rifle-ball—if they hit, they kill; if not, there is no record of the shot. There can be no good luck from scattering.

Now it will be perceived that the question of cures with high and low potencies is not merely a question of *potencies*, and our friend's trial will not be a *fair* one unless he make sure that his selection of the remedy in each case in which he tries the high potencies is strictly homœopathic. If he make sure of this and be correct in it, then let him go on in confidence with his experiment. We bid him God-speed.

THE QUESTION OF THE DOSE.

In a report to the Homœopathic Medical Society of the state of New-York, on the "Use of High Potencies in the Treatment of the Sick," published in this Review,[1] I stated, "that I have noted only one case in which a high potency acted but inefficiently, while a lower gave prompt and complete relief;" and, again, "that a general law for the *a priori* selection of the potency suitable for a concrete case, if such a law be possible, is yet to be discovered;" and, again, "this fact that a low potency succeeded where a higher had failed, together with similar facts reported by other practitioners, must have a bearing upon general conclusions."

The question of the dose is manifestly an open one. Experience must be accumulated before we can hope to discover a law for our guidance on the subject. Every fact bearing on the question should have our candid and careful study.

In the report alluded to, a portion of the evidence in favor of the high potencies was detailed. The following cases present evidence on the other side and deserve particular attention. They are published by Dr. Arnold, of Heidelberg, in the *Homœopathische Vierteljahrschrift*,[2] together with some very fair and ingenuous remarks by Dr. Arnold, who, it may be remarked, is in the habit of using only the lowest potencies or massive doses.

Psoriasis Guttata cured by Arsenic.—The power of the preparations of Arsenic to cure psoriasis is so well known that I should not publish this case, were it not that it fur-

[1] *North American Homœopathic Review*, vol. 4, p. 303.

[2] *Homœopathische Vierteljahrschrift*, January, 1864.

nishes, in addition, a striking evidence of the fact, that even in chronic diseases we are often obliged to give strong doses even of the very heroic remedies if we would accomplish a cure.

A lady, eighteen years old, whose childhood had been healthy, and who had never had any sickness worth naming, who felt perfectly strong and well, and had a blooming appearance, and in whom no predisposing cause for any skin disease could be discovered, observed several years ago, on certain parts of the body, isolated, red, somewhat elevated spots, on the surface of which small scales were visible. Inasmuch, however, as she felt well and the spots had spared the face and neck, she did not think it necessary to seek medical advice. Gradually the spots became more numerous, their dimensions also increased, and although the patient had no other complaint, she was yet induced to seek the advice of a physician on account of the itching, which was often troublesome, especially at night.

Purgatives and the so-called blood-purifying tea (species lignorum) had no result. In the spring of 1861 she came to me. The eruption was over the whole skin with the exception of the face and hands; the spots were large and confluent. I gave her at first one grain daily of the sixth decimal trituration of Arsenic. But as no change had occurred in fourteen days, I changed to the fourth decimal trituration, of which a grain was taken every forenoon. In fourteen days I found a slight improvement, in that certain spots seemed to be less red and did not itch so much at night. I now intermitted the use of Arsenic for three weeks, and after this period found again the old evil condition. One grain of the third decimal trituration of Arsenic, which was now given daily for fourteen days, wrought a more striking improvement, as well in relation to the development and size of the spots, which were much less, as also in respect to the itching at night which had almost altogether ceased. But inasmuch as, after a pause of fourteen days, this improve-

ment had again partly disappeared, I felt myself obliged to resort to a still stronger dose of the remedy in order not to put the patience of my client to too severe a test. She now received, once every day, two grains of the second decimal trituration—that is, one-fiftieth of a grain of Arsenious acid. The action of this dose resulted, after two weeks, in a very striking improvement, which moreover maintained itself during a suspension of the remedy for the next two weeks. I therefore allowed the patient to continue the remedy for two weeks longer, taking daily a two grain dose of the second decimal dilution. The cure was complete and permanent; for a half year afterward I had an opportunity of seeing the lady, and was assured that the recovery had been lasting.

On the subject of the homœopathic relation of Arsenic to psoriasis, scarcely any physician who is familiar with the effects of this remedy can entertain a doubt. I must confess, too, that I have often seen a cure result from the use of the sixth, but still more from the fourth decimal trituration of this drug. Even in the case just related, the amelioration might have proceeded to an enduring cure if the use of the fourth trituration had been continued for a longer time. But inasmuch as the patient had borne the Arsenic very well, and not a single symptom of its pathogenetic action had been apparent, even after a daily dose of two grains of the second trituration, there was no reason for losing time in the use of small doses. In any case, this is a new evidence that in chronic diseases even very heroic remedies must sometimes be given in very large doses.

Nasal Polypus cured by Calcarea.—A lady, aged fifty-five, of respectable position in society, pale, of delicate constitution, small and rachitic from childhood, but never seriously ill, perceived, several years ago, in the right nostril an impediment to respiration. An examination by a physician readily disclosed the presence of a mucous polypus. Various remedies were administered during a long period without

perceptible effect. Neither Mercurius dulcis, used as a snuff, nor Corrosive sublimate in solution, to be inhaled, had a noteworthy or enduring effect. The patient thought the slight changes noticed were rather to be ascribed to the weather, and especially to the dryness or moisture of the atmosphere than to the remedies used.

Under these circumstances, the operation being proposed, I was asked whether a cure was possible without operative procedures. I declared that a cure by means of internal remedies not only might succeed, but that it would have a much more enduring result than the mechanical removal of the growth could have; and furthermore, that an operation, in case it should prove necessary *after* an internal treatment, would be more certain and more lasting in its result than one without the previous use of corresponding remedies.

Having seen from Calcarea carbonica in several similar cases a very striking and unmistakable curative action, I prescribed the fourth decimal trituration of this remedy and ordered at first one grain daily, and after eight days two grains daily to be taken. After two months, the patient appeared and reported that she had taken the remedy four weeks and then intermitted it for four weeks, and that no change in her condition was observable. And I could myself perceive neither increase nor decrease of the polypus excrescence. This determined me to give the second decimal trituration of Calcarea, one grain daily. After fourteen days, the nostril having become more permeable, and a diminution in the size of the polypus being perceptible, although very trifling in degree, I allowed a pause of four weeks in the use of the remedy. After this lapse of time, the tumor had regained its former size; the lady was more than ever inclined toward the operation.

I concluded to wait upon her a few weeks longer, and gave her the officinal lime-water, a tea-spoonful twice daily in milk. Four weeks later the lady came again to me; a most careful investigation revealed no traces of the polypus. She

informed me that there was amelioration after the very first doses of lime-water, and that after a fortnight the nose had felt entirely free, and since that time there had been no aggravation.

The improvement has already lasted more than a year without any further administration of the remedy. Repeated examinations have failed to enable me to discover any trace of the polypus.

THE BASIS OF TREATMENT.[1]

Hahnemann, throughout his works, takes every opportunity to urge the insufficiency of a pathological theory of the nature of a disease as the basis of the treatment. He everywhere urges that the only sure indication for every case is to be found in the totality of the symptoms which the case presents. One would think that nothing could be more clear and convincing than his arguments on this subject.

His opponents declared that his method ignored medical science, left no scope for pathology and diagnosis, and reduced therapeutics to a degrading mechanical comparison of symptoms. Very many homœopathists have so far deviated from Hahnemann's method as to endeavor to blend with the use of his doses and remedies, an application of pathology as a basis of treatment. This endeavor can never be successful, inasmuch as the function of pathology is to furnish, not an indication for medical treatment, but simply a means of elucidating and collating the symptoms. The result has been a sad falling off from the standard of success in practice which was established by Hahnemann and his pupils. The points at issue are illustrated by the following case:

Willie M., four years old, was brought to me December 3, 1863. He had been healthy since February, 1863, when he is reported to have had a long attack of gastric fever, from which he finally recovered with the affection about to be described. This was a dyspnœa and wheezing, distinctly perceptible at all times when the child was awake, and

[1] Read before the Hom. Med. Society of Oneida County, N. Y., June 21st, 1864.

which, on making any exertion, were very much aggravated, and resulted in an attack of convulsive cough with difficult inhalation, the whole paroxysm resembling precisely what is described as Millar's asthma or *Laryngismus stridulus.* It was remarked that the child seemed to be free from dyspnœa when sleeping soundly, but at no other time. On waking in the morning he had always a hard fit of coughing, during which he sometimes raised a little tenacious mucus. His appetite was good, though somewhat capricious. He was considerably emaciated; his spirits were good, and he often attempted to join in the sports of other children, but was obliged soon to desist, because of the dyspnœa and cough which every physical exertion caused and which greatly fatigued him. On percussion and auscultation the lungs were found resonant; the respiratory murmur was, of course, masked by the loud wheezing.

The child had been taken, in September, to Professor A. Clark, of New-York, who, after careful and repeated examinations, had given a written diagnosis—"Chronic Laryngismus" He gave a very unfavorable prognosis and the advice to avoid all medication, save only a dose of some antispasmodic during the violent attacks of dyspnœa. This advice had not been followed. The child had been, throughout his illness, under what I regard as very skillful homœopathic treatment. I had once seen him in consultation, but had not been able to suggest anything that proved of service to him.

When now placed under my sole care, I well knew that the child had already taken, without benefit, every remedy which has symptoms at all resembling Millar's asthma or any spasmodic affection of the respiratory organs. And it was also evident, on even a cursory examination, that no one of these remedies was *clearly* indicated by the symptoms of the case.

I therefore resolved to follow, as implicitly as I could, the advice given by Hahnemann for the examination of the

patient and the selection of the remedy. Dismissing from my mind, then, every notion concerning the seat and probable pathological nature of the disease, I examined the patient and made the following record of the symptoms which he presented.

1. Child emaciated, flesh soft, skin inclined to be yellow (naturally fair—a blonde) and dry.

2. Appetite very good; always calls for food as soon as a coughing fit begins in the morning or forenoon.

3. The right hypochondrium hard, distended, tender to the touch, painful on exertion and when he coughs. The right shoulder is elevated and the spinal column laterally curved; dullness on percussion on the right side, extending three fingers' breadth below the margin of the ribs.

4. Distension of the epigastrium which is tympanitic on percussion, and tender to the touch.

5. Much rumbling of flatus in the abdomen.

6. Frequent ineffectual desire for stool; stool scanty and dry, occurring once daily or once in two days.

7. Cough dry; sometimes in the morning a very little tenacious sputa; always a coughing fit in the morning on waking; he has to sit up to cough; cough excited by eating and drinking, by rapid motion, by exertion, by crying or talking. The cough hurts his right side.

8. Constant wheezing and dyspnœa aggravated by exertion and by lying down, relieved during sleep.

The tender age of the patient rendered it impossible to obtain many subjective symptoms, such as usually facilitate the individualization of cases, and the determination of the appropriate remedy.

Before proceeding further in the narration of the case, I desire to say a word upon its pathology. The symptoms are before us: what shall our diagnosis be? Is the case one of spasmodic laryngeal disease, complicated by certain gastro-enteric and hepatic affections? Or, is it a chronic hepatitis, complicated by laryngismus? Which affection is

primary, and which secondary? What relation do the groups of symptoms bear to each other? Professor Clark seems to have adopted the former view, regarding the gastro-hepatic troubles as secondary, if indeed he paid any attention whatever to this complication. The homœopathic physicians who preceded me probably adopted the same view and based their treatment upon it. Now if in so doing they had happened to take a correct pathological view, the result *might* have been favorable; or if they had adopted and acted upon the second hypothesis, and this had chanced to be the correct view, the result *might* have been favorable.

But is it not obvious to every candid mind, that, in either case, success in the treatment based upon a pathological consideration of the case must depend on the correctness of the pathological hypothesis—a matter in which certainty can *never* be attained.

On the other hand, if we throw aside, as irrelevant, the entire series of questions as to which is the primary disease and which the secondary—which the original malady and which the complication—if we say to ourselves: "Here is a sick child; let us examine and record those points in which he differs from a healthy child," we get the series of symptoms above recited, which are *facts*, indisputable, unmistakable, the result of pure observation. If now, without hypothesis or speculation, we seek to find and do find a remedy which presents a series of symptoms corresponding closely to those of the patient, experience justifies us in believing that we shall have reached the utmost possible certainty of correctly selecting the remedy.

Comparing the symptoms with the Materia Medica, we perceive at once that the remedies whose names are usually associated with Millar's Asthma, Laryngismus, etc., viz.: Sambucus, Spongia, Cina, Lachesis, Hepar, Stannum, Chlorine, etc., etc., do not cover the case, having but little correspondence with groups 1, 2, 3, 4, 5, 6.

Nux vomica, on the other hand, covers these groups very well, as will be seen by comparing Materia Medica. In addition, it has violent paroxysms of cough in the morning very early (676 and 677), excited by motion and exertion (670, 671, 672), producing pain in the *epigastric zone* (689), and *accompanied by a desire to eat* (my own observation). It has also a well-marked dyspnœa.

The correspondence was so close that I had no hesitation in giving Nux vomica.

Dec. 9, four powders of the 200th were given, one to be taken every night, and the patient to report in ten days.

Dec. 21, the report was brought to me, that the child had no more wheezing nor dyspnœa; had been free from cough for five days, can play long and vigorously without inconvenience, is regular in his bowels, complains no longer of pain or tenderness in the hypochondrium—in fact seems to be perfectly well.

He deranged the digestion by eating candy at Christmas, and had a slight return of pain in the hypochondrium, which a dose of Sulphur relieved. He has ever since been entirely free from dyspnœa and laryngeal spasm, and is in the enjoyment of robust and perfect health.

If such a mode of practice as this be, as is charged, *unscientific*—if it ignore the sciences of pathology and diagnosis as bases of treatment—thus much at least may be said in its favor, that it far surpasses every other method in the facilities it affords for the fulfillment of one not unimportant object of the physician—the *cure of the patient.*

DIAGNOSIS IN HOMŒOPATHIC PRACTICE;

WITH COMPILATIONS FROM DR. KASPAR'S LECTURES.

It is an objection often urged against Homœopathy, that, as a system of therapeutics, it dispenses with the necessity for diagnosis. The objection is unfounded. For all purposes of prognosis and hygienic management, we have as great need of diagnosis as our brethren of the old practice. And so we have in *prescribing*. We profess to prescribe according to the "totality of the symptoms." But by this we are far from meaning to imply that each symptom is considered and prescribed for, independently of all the other symptoms. On the contrary, while on the one hand, *every* symptom must be taken into consideration as indispensable to a true "picture of the disease," and hence, in *this view*, no one symptom can be said to be less important than another; yet, on the other hand, the symptoms vary among themselves in rank. Some are primary or idiopathic, others are reflex or sympathetic. The latter are as important to a "picture of the disease" as the former, but they are important not *simply as symptoms*, but as *sympathetic symptoms*. Now, to form a diagnosis is to distinguish from among the whole number of symptoms, the *idiopathic* and *sympathetic* symptoms, to attribute each symptom, whether idiopathic or sympathetic, to the organ or tissue which is in reality its seat, and to form a just notion of the pathological condition of that organ or tissue. That it is necessary for us to make such a diagnosis before prescribing, follows at once from the rationale of a homœopathic prescription.

The homœopathic practitioner having got "a picture" of the disease he is about to treat, *i. e.*, having got the

"totality of the symptoms," proceeds to compare this picture with the pathogeneses of various drugs. Now, these pathogeneses are themselves "pictures of disease;" they are symptoms of drug-disease. As such, they are divisible into idiopathic and sympathetic symptoms; the organs, or tissues, in which they have their seat, and the pathological conditions of these organs, may be various. In seeking a drug whose symptoms shall correspond most nearly to those of the disease to be treated, it is evidently necessary to seek a drug, the idiopathic symptoms of which shall correspond to the idiopathic symptoms of the disease, and the sympathetic to the sympathetic; a drug, too, whose symptoms of either variety shall have the same seat as the analogous symptoms of the disease, and shall result from a *similar* pathological condition. But we cannot select this drug without having previously formed a diagnosis, not merely of the disease to be treated, but also of the various drug-diseases or pathogeneses that constitute our Materia Medica. This necessity may be illustrated by a reference to errors into which a neglect of it leads us. Spontaneous vomiting of bile by a child may be an idiopathic symptom indicating abdominal derangement; or, it may be merely sympathetic of cerebral disturbance. Several drugs produce vomiting of bile; some directly or idiopathically, by affecting the digestive organs; others, by a reflex or sympathetic action, their primary action being on the nervous centers. According to our diagnosis of the *disease* in the child, and of the *drug-disease*, we should give under the one view, Nux vomica, Ipecacuanha, or their analogues; or, under the other, Belladonna. Again, depraved appetite, convulsive movements, distorted vision, a peculiar aspect of distress, may be the idiopathic expression of disorder in the nervous centers, or may be sympathetic with irritation produced by entozoa. In the one case we should select a remedy which affects the nervous centers idiopathically, as Belladonna or its analogues; and in the other a remedy which acts idio-

pathically on the vegetative sphere, and affects the nervous centers sympathetically, as Cina. Neglect to distinguish between these varieties of symptoms has led compilers of manuals to recommend Cina *in hydrocephalus*.

Moreover, it is requisite to determine the *seat* of the symptoms, both in the disease and in the pathogenesis. Pain and tenderness in the right iliac region, with local heat and fever, may have their seat in the cellular tissue, in the muscular or peritoneal layers, in the cœcum, or even, at certain times, in the ovary. So might similar symptoms in a pathogenesis arise from a pathological condition of these various tissues Evidently we cannot select our remedy with certainty of its adaptedness, unless we make a diagnosis both of the disease and of the pathogeneses of the drugs.

Again, tenderness in the coxofemoral region, pain on moving the limb, and on pressing the great trochanter in toward the acetabulum, *may* have its seat in the tissues of the joint, or in the nerves that supply that region. And we have drugs that affect the tissues of the joint, and others that affect the nerves in question. We must select the former if the joint, and the latter if the nerves be affected; and in order to make the selection we must previously make the twofold diagnosis so often urged. Neglect of this has led to the erroneous recommendation of Colocynth in *hip-joint disease*. A twofold diagnosis, then, is a necessary preliminary to a properly conducted homœopathic prescription.

The diagnosis of the disease to be treated is to be made, of course, where the disease is encountered, at the bedside. But the diagnosis of the drug-diseases constitutes the systematic study of the Materia Medica.

Our provings are a mass of symptoms, for the systematic study of which some feasible method has been long a desideratum. Their study and arrangement under the form of a diagnosis furnish a method well calculated to fix the characteristics of the drug in the memory, and at the same time, as we have shown, practically useful to the prescriber.

Dr. Watzke and Dr. Kaspar, resident physicians in Wurmb's Homœopathic Hospital, in Vienna, have arranged a number of remedies in the form of a diagnosis, the exposition of which constituted the matter of lectures on Materia Medica, delivered by Dr. Kaspar at the Hospital, in the summer of 1851.[1] The substance of sixteen lectures by Dr. Kaspar follows, from which the scope and aim of the analysis will be evident; and it may not be amiss to suggest that these analyses are designed rather as aids in the study of Materia Medica than as *direct* guides for practice, and that, being greatly condensed, they require diligent study and constant comparison with the Hahnemannian pathogeneses.

NO. I. HEPAR SULPHURIS CALCAREUM.

Hepar has many close relations with Mercurius. The distinction between them is, in fact, rather negative than positive. Almost all the symptoms of Hepar are found under Mercurius, but not all those of Mercurius under Hepar.

Hepar, like Mercurius, develops its effects in the vegetative processes. Yet, whereas Mercurius increases to excess the whole secretive activity, and thereby produces emaciation (*i. e.*, by excessive secretion, salivation, diarrhœa, diaphoresis), the same character belongs to Hepar in a much less degree. Hence Hepar produces no putrid collapse, makes no penetrating impression upon, and alteration of, the vital processes; hence, too, it exerts no visible *reflex* action upon the *nervous life*, as Mercurius. While the resolutive, alterative action of Mercurius extends even to the formation of new secreting organs (abscess, secreting pus), Hepar possesses no such power. In the action of Hepar, appears, in a slighter degree, the tendency to *abnormal*

[1] These lectures the author attended.— ED.

secretion, and the resulting symptoms have, for the most part, quite a different pathological foundation from that of analogous symptoms of Mercurius.

The effects of Hepar appear most distinctly in the lymphatic system, the activity of which it excites in a high degree; and hence it either, on the one hand, increases the absorptive activity of the lymphatic system, or else, on the other, causes a too abundant collection of lymph in the glands, and, in consequence, obstruction, inflammation, and suppuration of the glands. We find, therefore, among the symptoms of Hepar, a great number indicating this character, and we find the greatest number of symptoms in those parts of the body in which the lymphatic vessels are most abundant, neck, shoulder, bend of the elbow, dorsum of the foot, etc.

On the increased resorption, on the other hand, depend a number of other symptoms in other organs, especially in those in which the lymphatic vascular system is highly developed.

Although I have so strongly insisted on *this*, as the sphere of action of Hepar, I would not be understood to deny to it any farther action. The excitation of organic activity can never be circumscribed to a few determinate functions.

If the resorptive activity of the lymphatic be increased, so will be also that of the venous system. The excretions will be altered in quantity and in quality.

The action of Hepar is upon the *periphery* directly. Hence we find congestion of the skin and mucous membranes; and, in consequence, redness, heat, swelling—in a word, an inflammatory condition shown by prickings, elevations of the skin, rhagades; a slight degree of suppuration manifesting itself in rhagades, pustules, nodes, tubercles, ulcers, also flashes of heat, and dull redness of parts rich in blood—lips, cheeks, conjunctiva, etc.

Partly in this way, and partly also by the fact of engorgement and obstruction of the lymphatic glands, we may explain the symptoms which appear in the *skin*.

Mucous Membranes. These are similarly excited. Their secretion is not greatly increased. Often, indeed, it is diminished; and it is *always thickened.* The *watery* portion being immediately absorbed again, the secretion becomes viscous, even *membranous.*

1. In the *Intestinal Tract,* Hepar must produce diminution of action; hence anorexia, nausea, vomiting, acid eructations, alienated taste, development of gas, etc., and, above all, enfeebled peristaltic action, and this, again, causes pain (meteorismus) and difficult evacuation even of soft fæces.

Add to these considerations, the irritation of the mucous membrane consequent on the great number of lymphatic vessels in it, and we may explain the several sensations and pains in the alimentary tract, especially the frequent tenesmus and aggravation after every stool. The stools are green, yellowish-brown, often mixed with blood.

2. In the *Respiratory Organs* we find a similar condition, various sensations indicating thickening of the secretions; hence, difficult respiration, laborious, dry cough. This condition, together with the puffy tumefaction of the mucous membrane, which usually accompanies such a condition, will explain to us the symptoms which we find especially in the larynx.

3. In the *Urinary Mucous Membrane* appears the same state of chemosis. Urine is scanty and dark, with abundant salts so that, when evacuated, it is already turbid, or else it soon becomes so.

4. The *Salivary Glands* are excited to a more abundant secretion.

The action of Hepar on the *Sanguine Vascular System* may be easily determined; for if it excites one portion of this system, the remaining portions *must* of necessity be also excited, though in a less degree. Hence, febrile symptoms are not wanting. They are still more clearly pronounced when a somewhat violent inflammatory excitement has manifested itself, producing its *reflex* action on the organism.

The general vascular excitement, however, can never assume the character of pure, intense synocha.

The relations of Hepar to the Sexual System are unimportant. The menses appear too early, and are too copious.

Sensations. As every revulsion of the vegetative processes provokes various sensations, so does that produced by Hepar. It is to be remarked, that *pains*,[1] strictly speaking, are seldom excited by it. The more frequent sensations are itching, pricking, rending, and feeling as if beaten. (In inflamed parts, however, burning, sticking, and tearing *pains* are experienced.) Such sensations will be especially felt in parts where lymphatic vessels course in greatest numbers, the axilla, bend of the elbow, etc.: thus we have heat, redness, and pain from the fingers to the shoulders; needle stickings, itching in the fingers, swelling in the fingers, and the same condition in the thigh, ham, dorsum of the foot and the toes.

Characteristic Symptoms.

1. Pains. Various. Especially *pressing* and *general sensations.*

2. *Aggravation of Condition.* At night, and by exposure to cold.

3. Thickening of the secretions, swelling, inflammation and suppuration of the glands.

Indications for Administration. Hepar may be indicated—

1. In diseases of acute, sub-acute, and chronic form.

2. In diseases occurring in lymphatico-phlegmatic individuals, with white delicate skin, blond hair, disposition to glandular swellings.

3. In all cases of glandular affection, as well for the collective chronic process as for the individual acute exacerbations, all scrofulous and tuberculous affections; enlarge-

[1] The Germans distinguish between Pain (*Schmerz*) and Sensation (*Gefühl*). *Pain* is *partial*, as to *extent*, and *determinate*, as to *seat*, *e.g.*, sticking in the shoulder. *Sensation* is general, as to *extent*, and indefinite, as to *seat*, *e.g.*, oppression, lassitude, itching.

ment, inflammation, and suppuration of all lymphatic glands, especially those of the neck; a similar condition of the cellular tissue. Hence it is indicated in mesenteric diseases of children; in frequently recurring angina; in inflammation of the salivary, cervical, and inguinal glands; panaris; inflammation of the eyelids; discharge of prostatic fluid.

4. All kinds of inflammation which pass into suppuration; purulent exudations (pleuritis); abscess (for maturation, Mercurius is preferable); hip-joint disease (caries).

5. Inflammation terminating in solid, membranous exudations; acute catarrh; laryngeal croup (after use of Aconite); lymphangioitis, as well external as uterine; phlegmasia alba dolens; hooping-cough, after the stage of inflammation (?).

6. Abscesses and analogous formations.

7. The *Mercurial Cachexy.* Calvities after Mercury; salivary fistula.

8. *Cutaneous Affections.* Crusta lactea; Tinea; Erysipelas (after Belladonna) in scrofulous individuals. Generally in scrofulous skin-diseases.

NO. II. CONIUM MACULATUM.

Conium belongs to that class of remedies which alter the vegetative vital process in all its relations. Among the vegetative functions, that of resorption suffers the first and most marked alteration, being diminished in activity or completely arrested. The great representative of this function is the lymphatic system, on which, therefore, Conium exerts its primary and most striking action.

Next in degree, and of the same nature, is its action on the venous system. The diminution of the vitality of this system involves, on the one hand, diminished general absorption, and on the other, retarded circulation of the absorbed fluids. Hence, collections form in parenchymata and cavities, either in the form of fluid infiltration or exu-

dation, or as hard tumors. In the lymphatics themselves depositions occur, which sometimes give rise to tumors. Such collections are hostile to the organism; they serve as excitants to the surrounding tissues; hence, an inflammatory condition not unfrequently arises in these formations, which leads to *their* destruction. In some cases, however, the contrary takes place, and the *organ attacked* becomes altered.

On these general pathological momenta depend the following Conium symptoms.

Skin. Tuberculous eruption, passing over into Furunculi; Petechiæ; Erysipelatous cutaneous inflammations; Gutta Rosacea; Ulcers; Gangrenous ulcers.

Glands. Various pains, generally sticking or cutting, especially in the mamma and mesenteric glands. Induration, suppuration, and alteration of the same; hordeola; swelling of the tonsils. Pains in the liver. Inflammation of the prepuce. Pressing in prostate, and discharge of prostatic juice. Orchitis; pain and swelling of testes. The various symptoms in the breast and abdomen indicate that the glands are attacked; but the fact needs anatomical demonstration. The *general* disturbance of the resorptive function probably depends on paralysis of the ganglionic system, in consequence of which *all the vegetative functions* must be more or less disturbed.

Vascular System. The vascular system loses its energy. The circulation becomes irregular, universally retarded and enfeebled. Whatever excitation there may be depends rather upon local causes than upon general stimulus. Hence, the general thermogenesis will be rather below the normal standard; but it *may* be *partially* increased. Consequently, in the Conium fever, coldness predominates; the pulse is slow and small. (Still, the contrary may occur; the pulse may be quick and strong, and the heat *partially* increased.) The blood in the venous system, therefore, will be propelled slowly; hence, this system will be gorged; this

will be especially manifest in the abdomen. The blood does not receive, on the one hand, the needed reparation, or, on the other, the necessary elaboration; it consequently tends to fluidity and decomposition. Accordingly, cyanosis, ecchymosis, ulceration, and hæmorrhage may easily and often be provoked. The results of every impediment of the circulation are manifested, rather in the *periphery*; hence, several inflammatory conditions produced by Conium. More frequently, however, the same cause favors the origin of infiltrations. By virtue of these tendencies we have inflammations of the eyes and eyelids, hordeola, inflammatory epistaxis, hæmorrhage from swollen livid gums, irritable state of the throat, bleeding ulcers, hæmaturia, and inflammatory symptoms in the urinary organs.

From what has been said of the *general* primary action of Conium, its reflex effect on the *individual functions* of the vegetative sphere necessarily follow. We mention the conditions of its action in *general* terms and in *specialties*.

The *vegetation* in general is *depressed*; this is shown by emaciation and change of color.

The *secretions* are in *general diminished*, because interstitial absorption is diminished; for secretion must necessarily proceed *pari passu* with absorption; and wherever this is not the case, the increase of secretion over absorption must be only in consequence of some *local* irritation, *e. g.*, increase of salivary, lachrymal, and prostatic secretions. Hence, vomiting is rarely produced by Conium. And, since Conium furnishes scanty secretions, the stool is, for the most part, retained and scanty. Urine is scanty; the mucous membrane of the urino-genital system is, for the most part, dry. The menses are retained or retarded.

As specialty, it may be remarked that the *intestines* stand in nearer relation to Conium than the stomach does (probably because of their more intimate connection with the mesenteric glands), and that the most various symptoms are produced in them. The stool is generally scanty, with flatulence and

pain before evacuation. If the *stools* are abundant and watery, so is the *urine.* The evacuation of urine is attended by violent pressing and by burning. The sweat is, for the most part, only partial, and is then accompanied by heat.

Nervous System The action of Conium on the nervous system is considerable, yet certainly of minor importance and often secondary, showing itself in spasms of various kinds; it centers in the sensorium, where depression predominates.

Sexual System The special relation of Conium to the sexual organs, especially the female, is very important. Its action is especially manifest in the breast and uterus, producing in these organs swelling, changes arrested, or abnormal secretion.

Condition The Conium symptoms are aggravated at night and early in the morning.

ANALYSIS.

1. Alters the *vegetative functions* by altering or modifying resorption; infiltrations or collections result, which tend to decomposition. Increased venosity, and finally depraved nutrition, effects especially evident in the lymphatic system and in all glands, and among these, *first,* the mesenteric.

2 *Glands.* In closest relation with female sex and mamma.

3. *Nervous System. Secondary Symptoms,* manifested by spasms.

4. *Secretions* In *general* diminished.

5 *Pains.* No particular character.

6. *Aggravation.* Night and early morning.

Application. Conium corresponds to lymphatic, blond, pale individuals; to children, especially those with large, soft abdomens, and tendency to glandular enlargements and cutaneous eruptions; to women, especially in connection with uterine diseases or with leucorrhœa; to women in the climacteric period; to old women with collections in the

uterus; to pregnant women; also to individuals who are easily excited; to hypochondriacs.

Diseases in general. All those dyscrasias whose development begins by deposition in the glandular system. Tuberculosis and scrofulosis, especially when these dyscrasias concentrate in the mesenteric glands; when, moreover, they evince rather a *passive* character, and do not run a rapid course, but gradually form deposits, and thereby give rise to partial congestions; in the first periods of such affections, so long as there is no especial colliquation, and while the secretions are in general diminished. (If, on the other hand, they show an *active* character, Hepar is indicated; when colliquation has set in, *Mercurius.*)

1. *Tuberculosis Meseraica.* Tuberculosis glandularum maxillarum colli. Angina in tuberculoso *without fever* (Conium alternated with Hepar).

2. *Scrofulous and Tuberculous Cutaneous Affections,* especially lupus sub-cutaneus; tubercles of the skin; papules with a white secrêtion.

3. *Scrofulous and Sub-acute Ophthalmia,* with *great photophobia* especially, affection and suppuration of the meibomian glands; formation of tubercles on the eyelids; conjunctivitis, with little or no mucous secretion; keratitis, especially when nebulæ form; also chronic nebulæ.

4. *Scrofulous Affection of the Ears,* with swelling of the parotids; albuminous, watery otorrhœa.

5. Scrofulous swelling of the nose. Ozæna.

6. *Scrofulous Spinal Affection.* Pott's curvature; softening of vertebral column; scrofulous coxalgia, which develops slowly; scrofulous and tuberculous caries.

7. In *Fibrous Scirrhus,* in so far at least as at the beginning, it consists of *albuminous infiltration,* Conium may be of service; but *generally* when an affection of this kind in the mammary or axillary gland is presented to our notice, it is too late for its removal. If, however, *rational* symptoms or hereditary taint give reason to anticipate or suspect the

commencement of such an affection, Conium would certainly be among the most useful remedies, yet the symptoms of Conium have rather the character of those of *infarctus* than of developed fibroides, in which the knife alone avails[1]

8 *Induration in general* of *mamma and uterus* (as well after reduction of inflammation as in chronic form), with discharge of mucus *All indurations;* not so markedly indicated, however, in indurations resulting from inflammation

9 For the *Carcinomatous diathesis* Conium can do nothing, even in the beginning

10 In Atrophy of mamma and uterus, Conium may be useful, the conditions to which Conium corresponds often coinciding with atrophy; numerous observations show its especial applicability to affections of old women

11 *Results of Contusions;* hence, after difficult or instrumental labor

12 Since the conditions already named are mostly conjoined with discharge, Conium may be considered as anti-leucorrhœic Chlorosis, amenorrhœa, and dysmenorrhœa, when they depend on the same cause, indicate Conium; yet these symptoms may be *independent*, and may still coincide with the symptoms of Conium To the same category belong also sterility, milk-tumors, and affections caused by the sudden removal of the child from the breast.

[1] The later English authors are disposed to confine the term Scirrhus to the first stage of *Cancer*, before ulceration takes place Scirrhus, then, is always a *malignant* affection. The Germans apply the term, however, indifferently to such benign or malignant tumors as from their physical character, *hardness*, merit the name. From this *character*, the *benign fibrous tumor* (*fibroides* of Rokitansky) of the uterus or mamma is often called *Scirrhus* It is to *this species* of tumor, and not to the *malignant Cancer in its first stage*, that Conium is applicable, and even in this, to benign fibroides only in the formative stage. During the year 1851, two patients in Dr. Wurmb's Hospital, in Vienna, in whom fibrous tumor of the uterus was clearly recognized were treated with *Conium*. In one, the tumor disappeared; in the other, it had greatly diminished in size, when she refused further treatment

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13. Affections of the *Intestinal* canal; seldom when these are *primary*,—chiefly when they are combined with, or rather are consequent upon, a general condition indicating Conium Vomiting during pregnancy; diseases of the intestinal canal in tuberculous and scrofulous persons, especially scrofulous children

14 Dry, nightly, tickling cough, especially in scrofulous or aged persons; evening dyspnœa, pertussis, with much vomiting; asthma senile

15 Feeble condition of old women and men; hysteria; hypochondriasis in chaste wedlock.

NO. III. MERCURIUS.

Mercurius has been well proved. Its symptoms are chiefly objective

1 Its only primary effects are on the vegetative sphere, which it affects in the highest degree, both quantitively and qualitatively It alters the vegetation so deeply and thoroughly, that it assimilates itself, as it were, to the whole organism, decomposes and separates its organic constituents, and becomes itself a new element of the organism Hence arises a new Mercurio-vital process, the natural vital process having given place to the operation of Mercurius All other effects—viz., upon the nervous system—are secondary.

2 In the vegetative sphere, Mercurius attacks, first of all, the secretive and resorptive processes (The increased absorption is not a secondary result of increased secretion. Absorption, as well as secretion, is primarily affected, as is shown by the rapid absorption of Mercurius by the external skin) It excites both of these processes to increased activity The secretions are therefore increased in quantity. They are altered in quality This alteration has reference both to consistence and to intimate chemical composition.

In consistence, they become thinner and more fluid; this effect distinguishes Mercurius from remedies otherwise analogous, but which thicken the secretions. (Hepar, Conium) As to chemical composition, they become acrid and excoriating This twofold alteration depends upon the peculiarity of Mercurius, already stated (1): in the first place, this plasticity of the secretions is diminished, and fluidation instead of coagulation is brought about; in the next, in the room of the displaced plastic materials, whose decomposition it effects, Mercurius insinuates and establishes itself.

3 Mercurius causes, first, a condition of excitation (even to inflammation and suppuration). This is to be explained, generally, by the stimulating action of Mercurius, and the reaction of the organism. Then follows a torpid collapse of the organic forces,—weakness even to exhaustion. This results from the commencing decomposition induced by Mercurius.

4 Mercurius penetrates the whole organism. No portion escapes its action Before all other parts, however, in susceptibility, stand: 1. All the membranes and the glands of the lymphatic system; 2. The parenchymatous organs; and 3. The muscular and nervous systems

EFFECTS IN GENERAL.

I. *Vegetation.* The vegetative function depends upon the equality in activity of the secretive and absorptive processes. If one or both of these be abnormally affected, nutrition and assimilation (vegetation) will immediately sympathize. Since Mercurius affects these processes primarily, in a very high degree, vegetation must be deeply affected. The vegetative process is retarded, and at last entirely arrested, with the following phenomena:

1. As to *Nutrition*. General emaciation. Laxity; pale, earthy hue of the skin and mucous membranes; baldness. Sponginess of the tissues, especially of the gums; disposition to hæmorrhage; diseases of the bones, and, as a

natural consequence, great weakness, increased by every slight effort; great sensibility to all injurious influences; violent thirst, double vision.

2. *Assimilation (i. e.,* with reference to vitality and constitution of the blood) is most deeply affected. On the one hand, the tone of the vessels is depressed; and on the other, the blood itself loses its plastic constituents, becoming altered in composition. Hence, in general, the pulse is increased in frequency, but is feeble (it may be also slow and soft). In the beginning, however, congestion and even inflammation occurs in individual organs; but later, only hyperæmia and stases in these and in the periphery. Exudations often occur, partly because of the local inflammation in the hyperæmic parts, and partly because of the increased fluidity of the blood. Although, in consequence of the decomposition of the blood, the venosity threatens to preponderate, this result is nevertheless obviated by the excessive secretion, and anæmia or hydræmia is everywhere manifest; or if venosity become manifest, it is of short duration, showing itself in ecchymosis.

Secondary Symptoms. With the phenomena above-mentioned, the whole vital energy succumbs. The thermogenesis will therefore be diminished. Thus, the chilliness, sensibility to cold air, and desire for the warmest possible temperature, are explicable. This is the general condition; there may be, however, temporary and partial excitement of the vascular system; and hence alternations of heat and cold occur, which appear so much the more frequently, as the erethistic condition is more highly developed.

II. *Nervous System.* The effects on the nervous system spring from two causes: (1), a general cause,—the diminution and decomposition of the fluids, and the reduction of the functions to an erethistic or even a torpid condition; and (2), a special cause,—the action of Mercurius upon the membranes, which extends to the neurilemma and the nervous centers.

1. The nervous power is diminished; hence, as regards sensation, a feeling of great depression, as well as actual prostration, restlessness and general indisposition; and, as regards motion, a lack of power, the motions becoming tremulous, indirect, and scarcely obedient to the will: hence, mercurial trembling, loss of speech, dysphagia, cough, etc.

2. By virtue of the special cause, the neurilemma being attacked, true neuralgia occurs (prosopalgia mercurialis), especially when single nerve-twigs are involved. The pain is drawing and tearing, attacks generally but a single nerve; often changes its location, but sometimes retains its seat for months; is aggravated by hygrometric changes, and by changes of temperature. The *left* side is the most powerfully affected.

3. As a general rule, only the nerves of sensation and motion are attacked, the vegetative nerves suffering through the local conditions.

4. The sensorium is generally depressed (save during erethism); hence giddiness, irritability, mental disturbance, mania, melancholy, loss of memory, imbecility.

III. *Secretions in general.* The secretions of every kind are altered and increased. They become acrid, and the quantity of some of their constituents is abnormally increased.

1. *Sweat.* Exceedingly copious, easily provoked, oily, sour, giving a yellow tinge to the linen.

2. *Of Mucous Membranes in general.* Acrid, causing therefore burning in the anus and tenesmus, tenesmus of the bladder, and burning, before and after micturition. The salivary secretion is characteristically increased. The pancreatic secretion is increased; secretion of bile is increased; hence greenish-yellow stools.

3. *Genital Organs.* Glandular and urethral gonorrhœa; increased discharge of semen. Menses increased in quantity. Leucorrhœa, sometimes mucous, sometimes purulent.

4. *Respiratory Organs.* The peculiarities of the secre-

tions induced by Mercurius are here less obvious. The normal secretion seems often even diminished. Frequent hæmorrhage.

Abnormal Secretions. To this category belong:

(1.) The purulent decomposition of inflamed parts generally.

(2.) Formation of pustular eruptions.

(3.) Ulcers in general, as combining these two characteristics. For, as to the former, the suppuration always proceeds from an inflammatory process, and is hence preceded by signs of inflammation,—redness, heat, swelling,—especially when occurring in the glands and cellular tissue; hence swelling of the gums, fauces, tonsils, epiglottis, parotid and submaxillary glands and inflammation and suppuration of the cervical, inguinal, meibomian and mammary glands. Again, as to the latter, the eruptions caused by Mercurius have the complete character of inflammation, with a tendency to suppuration or to puriform collections. (The irritative character of Mercurius, and the consequent reaction of the organism, together with the tendency of Mercurius to cause fluidity, induce the suppuration.) The pustule formation is often incomplete; hence erythema, eczema, erysipelas, impetigo, rupia (*large* vesicles); or, in mucous membranes, aphthæ, herpes præputialis. The eruptions have this peculiarity, that on the spots originally occupied by them, sequelæ form—*e. g.*, scales.

The mercurial ulcer originates in inflammation, removing the epidermis, and producing a discharge of pus and sanies. The ulcer rapidly increases in breadth and depth, soon becomes indolent and spongy, and finally bleeds. The circumference is red, the margins puffed. It most frequently occurs in mucous membranes, and in the mouth and on the genital organs.

5. *Serous and Fibrous Tissues.* These tissues become irritated. A secretion takes place in the capsules of joints and sheaths of muscles, fulfilling the conditions of rheu-

matism. In the serous sacs, collections of water form (mercurial rheumatism), and also of pus. Inflammation of the periosteum occurs, with deposition of new products, *e. g.*, gummata. The bones become dry and brittle, through loss of fluidity.

IV. Mercurius excites the lymphatic glandular system in general, even to inflammation and ulceration of the glands (axillary, cervical inguinal, etc.), and at the same time primarily stimulates in a very high degree, the absorption in the lymphatic vessels. N. B. It affects especially the salivary glands, increasing the secretion, inducing swelling, inflammation and suppuration.

The Liver. Mercurius induces *incomplete reproduction;* hence the fatty diathesis. It is indicated, before all other remedies, in fatty disease of the liver, resulting from depressed vegetation.

For the *Spleen*, Mercurius has little affinity. For the *Lungs*, more than for the spleen.

ANALYSIS IN ANATOMICAL ORDER.

Head. Various sensations and pains. Congestion.

Eyes. Irritation, swelling sensation as of sand in the eye, lachrymation, conjunctival congestion, photophobia, ptosis, œdema of cellular tissue and conjunctiva, loss of brilliancy in cornea, nebulæ, amblyopia, amaurosis, iritis.

Ears. Inflammation with pustules, hæmorrhage, deafness, illusion of the sense; pains.

Nose. Peculiar painfulness of nasal bones. Mercurius, above all remedies, is related to the nose, especially to the pharyngeal portion of it. Swelling and hæmorrhage.

Face. Collapse, paleness, blue rings around the eyes. Œdema, convulsive jerkings.

Teeth. Swelling, redness, burning, bleeding of gums; looseness of teeth, aggravation of pain by cold applications, and at night.

Mouth and Pharynx. At first, erythema; at the same

time œdema, vesicles, pustules, aphthæ. The inflamed spots are very sensitive and bleed easily. Swelling and dryness of tongue, discharge of saliva. Difficulty in moving the tongue, even to paralysis.

Throat, Œsophagus. Stiffness, swelling of pharynx, pressing pain, and excoriation. Dysphagia. The tonsils are first affected, then the gums.

Digestive Organs. 1. Loss of appetite, nausea, bulimy, metallic taste, no thirst, save a desire to moisten the dry parts affected. 2. *Digestion disturbed.* Ructus, vomituritis, collection of water in the mouth; nightly nausea. Mucous secretion increased and changed; pancreatic juice altered. Stomach pains. 3. *Abdomen* Fullness, oppression, meteorism, sensibility, sticking, cutting, pinching pains in liver. 4. *Stools.* Frequent, with little or no discharge of solid or soft fæces; pappy, whitish-gray or green-yellow, sometimes bloody, acrid and excoriating; tenesmus, pinching and burning in anus, going on to inflammation, suppuration and hæmorrhage. Dysentery.

Mercurius has especial affinity for the two ends of the digestive canal. The stomach and upper intestine are but slightly affected; often not at all.

Genital Organs. The mucous membrane is the part chiefly affected.

1. *Male.* Swelling; formation of scales, vesicles, pustules, ulcers, analogous to those of the mouth and pharynx; discharges, itching and burning; discharge of semen without pleasure; gonorrhœa.

2. *Female.* Œdema: Leucorrhœa purulent, acrid. *Menses, copious.*

Respiratory Organs. Expectoration streaked with blood. Mucous membrane light-colored; its substance little affected. Coryza, with acrid, purulent discharge. *Trachea.* Dry irritation; cough, with bloody sputa. *Lungs,* dyspnœa, asthma, sticking pains, oppression.

Back and Limbs. In these parts are experienced all the

symptoms relating to skin, serous and fibrous membranes and bones, *e.g.*, itching, crawling, burning, lassitude, loss of power—tearing, sticking, and throbbing pain, cracking of the joints—cramps and trembling—œdema, exostosis.

Characteristics. Aggravation by cold, and at night; from exposure to currents of air, and to change of weather; this applies to *all* the symptoms. Swelling and irritation of the parts attacked, suppuration, ulceration, pustular eruptions. Increased and acrid secretions, salivation, diarrhœa, gonorrhœa. Menses too copious. Sweat; trembling and spasm, especially of the flexors. Osseous affections. Especial relation to those mucous surfaces which are nearest the external skin.

Application. 1. *Generally.* To individuals of a depressed vegetation, of a tendency to mucous and bloody discharge, to suppuration with hydræmia, to affections of the lymphatic and glandular systems. To children in the period of dentition; to youths of feeble and leucophlegmatic temperament, but *not* torpid, on the contrary, rather erethistic. In both acute and chronic diseases; in the former, however, not until the general excitement has somewhat abated, and the inflammatory condition has become localized. In simple inflammations as well as in formation of deposits.

2. *As to crases.* In all crases characterized by slow depression of the plastic life, by increased and altered secretions, and still more, by a tendency to suppuration or to purulent exudation. All crases in which Mercurius is indicated are characterized by *erethism* and never by *torpor*, hence, Mercurius does not stand high on the list of antiscorbutics, because in scorbutus there is mere collapse, and no tendency to suppuration. Still in cases of scorbutus, with manifest excitement combined with phlebitis, it is often available. In other cases, Carbo vegetabilis, Arsenicum, China, Arnica, Lachesis, are preferable. Mercurius is more strongly indicated in the tuberculous and scrofulous crases when the parts attacked threaten to inflame and suppurate,

and where erethism is present and ulceration threatens. As to *special crases*:

1. *In Tuberculosis.* In the second stage, with tuberculous ulceration and copious purulent expectoration, with fever and gradual emaciation, Mercurius should *never* be employed. It is indicated only in an erethistic condition of the lungs—hence *not* in pneumonia, rather in hectic fever in consequence of pyæmia. In tuberculous affections of the bones with tendency to caries. In necrosis.

2. *In Scrofulosis,* with ulceration or swelling of the glands, otherwise Calcarea is preferable. Scrofulous affections of the bones. Scrofulous ophthalmia. In *rachitis,* as distinct from scrofula, probably not so appropriate as Calcarea

3. *In Syphilis,* especially when the mucous membrane and glands are affected—especially when ulcerated. Mercurius has less affinity for the eruptions unless they be pustular (Rupia). Not so well adapted to the syphilitic osseous affections.

4. *In Pyæmia,* from absorption of purulent matters. Except Arsenic, no better remedy is known for this crasis. Also in crases that present similar symptoms to those of pyæmia.

5. *In the Inflammatory crasis,* with mild fever with tendency to localization, and inflammatory exudations in the cellular tissue and glands Pleuritis, meningitis, peritonitis. Inflammation of the capsules of joints and sheaths of muscles, with purulent, plastic exudations.

6. *In the Typhoid crasis,* Mercurius is scarcely indicated unless in typhoid parotitis. But in puerperal fever it is a most precious remedy against both the local affection and the general condition. In metritis puerperalis,—affections of the joints, and puerperal deposits (metastatic), because of the general tendency to pyæmia.

7. *In the Rheumatic crasis,* seldom indicated, rather where the periosteum is affected with aggravation by change of weather.

8. *In the Œdematic crasis*, contra-indicated in anasarca or ascites.

9. Mercurius is indicated in *Icterus*, with irritation of the liver—febris biliosa.

10. *In Exanthemata*, vesicular, and pustular (seldom the papular), both acute and chronic. In variola, when pyæmia is established after the repercussion of the eruption. In salivation; in herpes zoster Mercurius vivus is a specific. In benign furunculus (not in malignant), impetigo, miliaria, eczema, crusta lactea.

INDICATIONS IN ANATOMICAL ORDER.

1. *Head.* Hydrocephalus acutus; abscesses in the scalp; caries; syphilis.

2. Ophthalmia scrofulosa, and catarrhalis with purulent secretion; inflamed cellular tissue.

3. Otitis with purulent discharge.

4. Angina with tendency to suppuration. Tonsillitis. Aphthæ.

5. Swelling, inflammation and suppuration of the gums.

6. Glossitis; inflammation of cellular tissue and glands, with irritation of all surrounding parts; suppuration, ulceration, salivation, toothache, with ulcers from caries, stomacace (compare Iodine).

7. Hepatitis, acute and chronic. Bilious derangements.

8. Enteritis, acute and chronic, when actual pus is formed after the first stage.

9. Diarrhœa generally, with muco-purulent stools, green and bloody, tenesmus, scanty stools, much tormina, burning in rectum, *no pains in the abdomen.*

10. Proctitis, and proctalgia, with discharge of pus and blood.

11. Dysentery in *high* grades, with purulent secretions, tenesmus, scanty stool.

12. Inflammation of kidneys (and irritation after inflammation), with irritation and purulent urine.

13. Inflammation of the bladder and urethra.

14. Gonorrhœa, ulceration of the genitals; œdem præputialis (especially in children, with slight erysipelas) orchitis; leucorrhœa excoriating.

15. Metritis, especially lymphangioitis, leading to pyæmi

16. Oophoritis (after Belladonna, and succeeded b Platina). Hydrops ovarii. (Graphites ?)

17. Catarrh; *not* indicated unless there be purulent dis charge.

18. Myelitis.

19. Panaritis. (Mercurius yields only to Hepar i efficacy.)

20. Psoitis; scrofulous coxalgia in incipience. Whe suppuration already exists, Mercurius does not remove i Phlegmasia alba dolens.

21. Arthritis. White swelling.

22. Trembling, twitching of the features, as a secondar phenomenon. Neuralgia.

NO. IV. ACONITE.

Aconite is the general representative of antiphlogistics.

Effects in general. 1. *Vascular System.* It operates powerfully as a direct excitant of the vessels, and produces, therefore, true synocha.

2. Aconite produces no *crasis*, and has, therefore, no tendency to product-formation. Its action is purely dynamic, viz.: the pulse is much accelerated, hard and strong; thermogenesis is increased; thirst great; general sensation abnormal.

3. It has a greater action on the more vascular organs—hence above all, on the lungs, giving rise to various degrees of excitement; next to the lungs it affects the periphery, and, last of all, the intestines. Hence congestion of blood to these parts, and hæmorrhages.

4. It has no especial relations to the fibrous and mucous tissues, although it exerts an inflammatory action on all organs and tissues of the body. (Bryonia also has this specific action; although it excites the vascular system less intensely than Aconite.)

5. Its action on the *Nervous System* is secondary. 1. The sensorium is either *excited* in consequence of the vascular excitement,—hence delirium,—or, 2. It is depressed by reason of the vascular congestion; hence sopor.

6. Aconite has characteristic morning and evening exacerbations; the majority of the pains are *sticking;* other pains occur according to the nature of the organ attacked.

General Indications. Aconite is generally indicated, 1. In all inflammations in the beginning, against the vascular excitement which always prevails before the disease is fully developed. Its action is brief, and does not interfere with that of subsequent prescriptions. 2. As an alternating remedy after the development of the disease, when the vascular excitement continues.

Special Indications. 1. (1.) Inflammatory Fever. (2.) In all inflammations. a. While the vascular excitement is great, and before any product has yet formed. b. When the product is already forming, if the vascular storm has not abated. (3.) Especially in inflammations of the serous and fibrous tissues; Aconite is preferable to all other remedies in these cases—when the vascular excitement is great, and a *plastic* exudation threatens (for less intense vascular excitement and a threatened *serous* exudation, Bryonia); hence in meningitis, pleuritis, peritonitis, under these conditions.

2. In *Ophthalmia,*—acute, with febrile excitement,—a sovereign remedy.

3. In the periods of dentition and menstruation with vascular excitement. It does not disturb the menstrual function.

4. Acute Rheumatism.

5. Acute Catarrh.

6. Tuberculosis with febrile excitement.

7. Vascular affections Arteritis, pericarditis, and endocarditis. In organic heart disease, for the periodical excitement. (Aconite is preferable to Digitalis, which sometimes leaves injurious sequelæ.)

8. Violent dynamic congestions, depending on no *organic* cause.

NO. V. CALCAREA CARBONICA.

Calcarea affects exclusively the vegetative system; all other action is secondary. The secretive and resorptive functions are immediately stimulated to increased activity, and a condition of irritation ensues. All organs and systems are affected, the nutrition of all being altered The lymphatic system is affected in a special degree; and, in the mode of this affection, the Calcarea symptoms resemble the scrofulous process in its lower grades. Calcarea is a chronic remedy, because it alters the crases; also a subacute, because the *products* of the altered crases provoke a state of irritation. Its action is not stormy, but mild and slow.

1. *Nervous System.* Not specifically affected; the isolated spasms that occur are secondary. Pains corresponding to the nature of the organs attacked. Sticking pains, and a sensation of rigidity, predominate.

2. *Vegetation in General.* A cachectic habit is induced; decrease of temperature; puffiness of the soft parts; laxity and general debility, with increased sensibility to external influences,—as changes of weather.

3. *Vascular System.* Moderate degree of fever; chill and heat quickly alternating; fugitive heats; chilliness predominates. Sweat breaks out easily, especially in the palms and soles; evening exacerbation. Thus, a fever resembling that of Tuberculosis. The composition of the blood is not *primarily* altered; *secondarily* it *is* altered.